THE MATHEMATICS OF COLLISION AND THE COLLISION OF MATHEMATICS IN THE 17TH CENTURY

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To Patty & Thea

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This dissertation charts the development of the quantitative rules of collision in the 17th century. These were central to the mathematization of nature, offering natural philosophy a framework to explain all the changes of nature in terms of the size and speed of bodies in motion. The mathematization of nature is a classic thesis in the history of early modern science. However, the significance of the dynamism within mathematics should not be neglected. One important change was the emergence of a new language of nature, an algebraic physico-mathematics, whose development was intertwined with the rules of collision. The symbolic equations provided a unified system to express previously diverse kinds of collision with a new representation of speed with direction, while at the same time collision provided a practical justification of the otherwise "impossible" negative numbers. In private manuscripts, Huygens criticized Descartes's rules of collision with heuristic use of Cartesian symbolic algebra. After he successfully predicted the outcomes of experiments using algebraic calculations at an early meeting of the Royal Society, Wallis and Wren extended the algebraic investigations in their published works. In addition to the impact of the changes in mathematics itself, the rules of collision were shaped by the inventive use of principles formulated by 'thinking with objects,' such as the balance and the pendulum. The former provided an initial framework to relate the speeds and sizes of bodies, and the latter was key both in the development of novel conservation principles and made possible experimental investigations of collision.

This dissertation documents the formation of concepts central to modern physical science, and re-evaluates the mathematics of collision, with implications for our understanding of major figures in early modern science, such as Descartes and Huygens, and repercussions for the mathematization of nature.

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Chapter 1
Introduction

Indeed, if from history one learned nothing else than the variability of views, it would be invaluable. Of science, more than anything else, Heraclitus' words are true: 'One can not go up the same stream twice.' Attempts to fix the fair moment by means of textbooks have always failed. Let us, then, early get used to the fact that science is unfinished, variable.

—Ernst Mach, *The History and Root of the Principle of the Conservation of Energy* (1872)

The natural philosophical understanding of the world was changing in the early 17th century. Specifically, how change itself was understood, was changing. In other words, we see a development of new kinds of explanations of the changes occurring in nature. Collision—two bodies bumping into each other—and the *mathematical* study of collision are key components in these developments. For example, the qualitative Aristotelian account of change, which appealed to the four elements (earth, air, water, fire) and the four qualities (hot, cold, wet, dry), was in contest with various versions of the mechanical philosophy, which appealed to little more than matter and motion—phenomena are explained by reducing them to unobservable bits of matter moving and interacting through contact.

Thomas Harriot claimed that his porisms on the reflection of bodies "lead together towards the innermost Mystery, or the understanding of Natural Philosophy."[1] Isaac Beeckman studied collision to better understand the behavior of the corpuscles in his physico-mathematical explanations of natural phenomena. And René Descartes presented his rules of collision as an extension of his third law of nature, the impact law; they constitute the fundamental principles of his natural philosophy. Similarly, according to Christiaan Huygens, "nature consists of certain particles, from the motion of which all the diversity of things arises, and by the extremely rapid impulse of which light is propagated and spreads through the immense spaces of the heavens in a moment of time, [...] this examination [of nature] will seem to be helped no small amount if the true laws by which motion is transferred from body to body be made known."[2] Thomas Sprat, an early member of the Royal Society, and its first historian, highlighted Christopher Wren's

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[1] HMC 241/VIA f. 23^r.
[2] HOC 16: 150. Translation by Richard Westfall, *Force in Newton's Physics: The Science of Dynamics in the Seventeenth Century* (New York: American Elsevier, 1971), 147.

"doctrine of motion," which was "propos'd as the Principles of all *Demonstrations* in *Natural Philosophy:* Nor can it seem strange, that these *Elements* should be of such Universal use; if we consider that *Generation, Corruption, Alteration,* and all the Vicissitudes of *Nature*, are nothing else but the effects arising from the meeting of little Bodies, of differing Figures, Magnitudes, and Velocities."[3]

Establishing the mathematical rules of collision was a central problem that bridged several areas and methods of investigation, including physical optics, the principles of natural philosophy, mechanics, and experimental philosophy. The recent invention of the telescope, and Harriot's and Galileo's astronomical observations, had sparked renewed interest in lenses, their optical properties, and the precise description and explanation of refraction and reflection. Harriot's account of collision in *De reflexione corporum rotundorum* was likely intended as a physical explanation of light. There are several connections between Descartes's impact law and his explanations of reflection and refraction. And Huygens highlighted "the extremely rapid impulse of which light is propagated" as one of the "diversity of things [that] arises" from the collision of particles. Although the motion of particles could explain the behavior of light, Descartes's rules of collision were given a place prior to this particular context, among the principles of his philosophy. Huygens's *De motu corporum ex percussione* was primarily inspired by the Galilean tradition of mechanics, and Beeckman's explanation of the corpuscles in his physico-mathematics relied on the pseudo-Aristotelian *Mechanical Problems.* Christopher Wren and John Wallis brought the

[3] Thomas Sprat, *The History of the Royal Society of London* (London: T. R. for J. Martyn and J. Allestry, 1667), 312.

investigation of collision into the domain of the new experimental philosophy at the Royal Society to establish what they called the laws of nature.

Mathematics, and the perceived relationship between mathematics and the world, was also changing. Several interpretations of mathematics were in contact, influencing each other. For example, the two pillars of mathematics, arithmetic and geometry, were about different things: the subject matter of arithmetic was discrete quantity, whereas geometry was about continuous quantity. They were for different purposes: arithmetic was a tool of calculation; geometry was a system in which one could provide demonstrations of propositions using only a handful of first principles. Of course, practically, before a person can prove something they usually first go through a process of discovery—they work backwards, and figure out what all of the right pieces are, before putting them into place in a demonstration. The guide to this process was called the analytic method. Many in the 17th century believed that the ancient Greeks must have had this method in order to write such sophisticated synthetic proofs (where all the pieces are put back together), but they had for one reason or another suppressed it. One of the innovations of the 17^{th} century was Descartes's "analytic geometry" or algebra of lines. The symbolic calculating techniques of a kind of sophisticated arithmetic were brought together with geometry as a problem-solving tool. And many believed that algebra must be the lost ancient method of analysis. Two kinds of mathematics came together, and through interaction, algebra became understood as analytic geometry. Combining these two, one could then represent lines by symbolic equations, which rely on the operators of arithmetic.

The fields of mathematics were diverse and changing in the 17th century. In general, the studies of collision can be construed as part of "physico-mathematics," or in the metaphor used in the 17th century, the language of the book of nature. This is distinct from pure mathematics, which was abstract, or practical mathematics, which was primarily instrumental, although both of these play their part in the development of the rules of collision. The various fields and subfields of mathematics, such as arithmetic, geometry, and algebra, were increasing in number. John Dee, for example, categorized 21 "Sciences and Artes Mathematicall" in his *Mathematicall Præface* to the first English edition of Euclid's *Elements.*[4] In addition to these fields, there were several different "schools" of mathematics, such as the classical traditions stemming from Euclid, Archimedes, or the pseudo-Aristotelian *Mechanical Problems*, or the traditions of 16th and 17th century "practical mathematics" supported by Robert Recorde, John Dee, Thomas Diggs, and Petrus Ramus, or the tradition of analysts such as François Viète, William Oughtred, and René Descartes. Along with these fields and traditions, there were tensions among these differences, thus a "collision of mathematics."

The changes that mathematics was undergoing, and the particularities of these fields and traditions, impacted the development of the rules of collision, just as the rules of collision would have an impact on the interpretation of mathematics, namely the interpretation of negative quantities. Contrary to past historians, who have presumed that the technical aspects of mathematics can be avoided to focus on the conceptual

[4] John Dee included a foldout insert with his preface to first English translation of Euclid's *Elements* (1570), which contained an elaborate taxonomy of the varieties of mathematics. Calling this chart his "Groundplat of my MATHEMATICALL Præface," he elaborated on each of the twenty-one "Sciences and Artes Mathematicall" in the preface itself.

development of science, we will see that the technicalities of mathematics (some of them quite simple) are intrinsic to the conceptual development of science.[5]

As the natural world was increasingly being investigated mathematically, mathematics itself was developing. The rules of collision are a focal point of these changes. Not only did the 17th century see a "mathematization of nature," but there were significant changes in the mathematics of nature. Specifically, developing in tandem with the rules of collision was the emergence of an algebraic physico-mathematics. This new mathematics of nature made possible novel conceptualizations of the directionality of motion, and provided a unified expression of collision in a system of equations with two unknowns. The arithmetic-algebraic operators $+$ and $-$ provide a new expression of contrary motion, and conversely the contrary motions involved in collision would provide (in the work of John Wallis, for example) a legitimation of heretofore "impossible quantities" such as negative numbers.

This dissertation focuses on the contemporary mathematics of the seventeenth century. I do not ignore the technical aspects of mathematics as if they were irrelevant to the conceptual development of science. And as much as possible, I do not reconstruct the seventeenth century theories of collision in modern mathematics, as if mathematics were a purely neutral language. Using this historically sensitive methodology, I provide

[5] Richard Westfall, for example, explains that in his history of ideas, he "attempted to define the problems on which they expended themselves in their terms, and to see their proposed solutions in relation to the intellectual equipment at their disposal." However, he does not consider the technical aspects of mathematics part of the conceptual development. "Whereas I devote no attention to social factors, I devote very little more to technical mathematical questions. I do not mean to deny in any way the importance of mathematics in seventeenth-century dynamics. With the calculus, for example, a whole new range of problems hitherto beyond the grasp of quantitative mechanics became amendable to exact treatment. My central concern has focused on conceptual issues, however; and during the development of dynamics up to Newton, such matters appear to me to have been central to the science of dynamics." Westfall, *Force in Newton's Physics*, ix-xi.

significant reinterpretations of Descartes's rules of collision and the development of Huygens's theory, and articulate an important change in the mathematization of nature.

Descartes's works on collision and mathematics are of pivotal importance. His conservation principle, the "impact law," and the 7 rules of collision in the *Principles of Philosophy* framed a new vision of the world. It set the topic—the mathematical study of collision—as an area of investigation, and inspired the hopes of the next generation of thinkers who sought to explain all the changes of nature in terms of quantitative rules of collision. They accepted the general manner of posing the problem, but were critical of Descartes's results and the means of arriving at those results. Although Descartes did not use the symbolic algebra from his analytic geometry in his physics, his immediate successors would do so fruitfully, with devastating consequences for Descartes's own theory of collision. Because of its central importance, a historiographical essay on Descartes's physics has been included as an appendix to the dissertation.

Descartes's mathematics also played a pivotal and ironic role in the emergence of an algebraic physico-mathematics. Beeckman investigated the apparent "destruction" of motion in the collision of bodies, using the proportions from the pseudo-Aristotelian balance. Rather than focus on the loss of motion, Beeckman's colleague, Descartes, changed the emphasis to the "quantity of motion," and put the conservation of this quantity at the heart of his system. Nevertheless, the mathematics of Descartes's early view of collision is strikingly similar to Beeckman's use of the pseudo-Aristotelian balance. Descartes developed analytic geometry, but did not use his symbolic algebra in his physics. However, with attention to the historicity of the mathematics in the *Principia philosophia*, we find the classical analytic method underlying Descartes's rules of collision. Huygens, in his early manuscripts, then uses Descartes's symbolic algebra to criticize Descartes's theory of collision—he shows that Cartesian quantity of motion is not conserved and the rules are inconsistent. Huygens's heuristic work with algebra likely served as scaffolding for a new principle, the conservation of Cartesian quantity of motion with direction. He also used symbolic algebra at the Royal Society to predict the outcomes of experiments with colliding pendulum bobs. Huygens's algebra remained in his manuscripts, and he formulated his theory of collision in the axiomatic tradition of Archimedes and Galileo, using classical conceptions of quantities. Huygens deliberately avoided the production of negative quantities in the algebraic investigation of collision in his manuscripts. Two of the members who were present at Huygens's successful predictions, John Wallis and Christopher Wren, would go on to developed their own theories of collision. They not only expressed them algebraically, but Wallis would use the notion of contrary motion, which is central in investigations of collision, to legitimize the notion of a negative number, and Wren would claim that nature itself obeys the algebraic rules of addition and subtraction.

The dissertation focuses on the theories of collision by Isaac Beeckman, René Descartes, Christiaan Huygens, Christopher Wren, and John Wallis. There are a set of interpersonal links directly connecting their ideas, but more importantly their works each had a role in the emergence of an algebraic physico-mathematics. Thomas Harriot, as he is in many ways, is an exception. His draft *De reflexione corporum rotundorum* brought together geometric diagrams, symbolic equations, and the study of collision before anyone else. But he did so in a way that is strikingly different from his successors. He did not publish his account, and none of his successors appear to have been familiar with *De* *reflexione.* His work is included in this study to show, through contrast, the different ways mathematics was used to express physical quantities such as motion and body in collision. It also shows that the pairing of positive and negative signs directly together with the directionality of motion was not obvious, nor should it be taken for granted.

Each chapter compares and contrasts two sets of work. In chapter 2, Harriot's theory of collision from his manuscripts is paired with Beeckman's thoughts on collision from his *Journal*. Chapter 3 compares Descartes's early view of collision from his correspondence and *Le Monde* with Descartes's later view of collision in the *Principia philosophia*. With attention on this important shift, and the historicity of his mathematics, I provide a new interpretation of Descartes's rules of collision. Chapter 4 compares Huygens's work with that of Descartes. It emphasizes Huygens's use of Cartesian symbolic algebra against Descartes's physics in Huygens's manuscripts, and the subsequent axiomatic formulation of Huygens's theory in the tradition of Archimedes and Galileo. And chapter 5 compares the theories of Wallis and Wren at the Royal society, highlighting their experiments, and their published algebraic laws of motion.

Chapter 2
First investigations: Harriot and Beeckman on collision

I have now led you to the doors of nature's house, wherein lie its mysteries. If you cannot enter because [the doors] are too narrow, then abstract and contract yourself into an atom, and you will enter easily. And when later you come out again, tell me what wonders you saw.

—Harriot to Kepler, 2 December 1606

CHAPTER 2 OUTLINE

Introduction

Harriot's manuscript, *De reflexione corporum rotundorum*, and Beeckman's studies in his *Journal* are among the first mathematical investigations of collision. Elsewhere in their works, both relied on the motion and interaction of the smallest bodies in their explanations of natural phenomena. Their investigations of collision each turn the focus from collision as a fruitful *explanans* to collision as *explanandum*.

Harriot's work brought symbolic equations together with a geometric diagram to understand collision. Although *De reflexione* was not directly influential on the development of the rules of collision, it serves as an important contrast class. In this text we see a possible combination of key mathematical concepts with collision, but one which ultimately did not prevail. Examining Harriot's unique study of collision highlights the significance of fundamental concepts, such as the roles of positive and negative signs to indicate direction, that were not immediately recognized. Harriot's work also underscores the importance of the historical mathematical concepts in the formation of theories of collision.

Beeckman wrote on collision not to establish rules or express a principle of conservation, but rather, to investigate what he considered to be a problematic puzzle in his own corpuscular natural philosophy. The manner in which he investigated collision relied on the relations of quantities from the pseudo-Aristotelian account of the balance.

Section 1
Thomas Harriot on the Reflection of Round Bodies

1.1 – Kepler's question: Thomas Harriot's physical explanation of light

Thomas Harriot (1560-1621) explained why it is important to understand the "reflection of round bodies" on the first folio of his manuscript, *De reflexione corporum rotundorum, Poristica duo* (1619).[6] By understanding this "dignified" and "extraordinary" topic one will be guided towards the "innermost Mystery, or the understanding of Natural Philosophy."[7] Harriot's "Mystery" was likely the underlying nature of light.[8]

In October 1606 Johannes Kepler wrote to Harriot to discuss optics.[9] Two years prior, Kepler had published the *Astronomiae pars optica* (1604). But, as he explained in

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[6] The history of Harriot's manuscripts is complex, and is tied to the changing status of his reputation, as well as the changing assessment of the measure of the influence of his unpublished ideas. See Gordon Batho, "Thomas Harriot's manuscripts," in *Thomas Harriot: An Elizabethan Man of Science*, ed. by Robert Fox (Burlington: Ashgate Publishing, 2000), 287-297. Matthias Schemmel, *The English Galileo: Thomas Harriot's Work on Motion as an Example of Preclassical Mechanics*, 2 vols. (Berlin: Springer, 2008), 245-755. Shirley, *Biography*, 1-37. Jacqueline Stedall, *A Discourse Concerning Algebra: English Algebra to 1685*, (New York: Oxford University Press, 2002), 97-100, 111-125. Stedall, *Great Invention*, 3-34. Stevens, *Hariot the Mathematician, Philosopher, & Scholar*, 161-192, 193-203. R. C. H. Tanner, "Henry Stevens and the Associates of Thomas Harriot," in *Thomas Harriot: Renaissance Scientist*, ed. by John W. Shirley (Oxford: Clarendon Press) 91. R. C. H. Tanner, "The Study of Thomas Harriots' Manuscripts: I. Harriot's Will," *History of Science* 6 (1967): 1-16. R. C. H. Tanner, "The Study of Thomas Harriots' Manuscripts: II. Harriot's Unpublished Papers," *History of Science*, 6 (1967): 17-40.

[7] HMC 241/VIA f. 23r. *De reflexione corporum rotundorum, Poristica duo*. "With dignity they [the porisms] are among extraordinary matters, which lead together towards the innermost Mystery, or the understanding of Natural Philosophy." *Sunt etiam dignitate inter præcipuam quæ ad Naturalis philosophiæ penetralia siuè Mysteria conducunt intelligenda.* Also see the translations in the following three classic papers on Thomas Harriot's *De reflexione*. Martin Kalmar, "Thomas Hariot's *De Reflexione Corporum Rotundorum:* An Early Solution to the Problem of Impact," *Archive for History of Exact Sciences* 16 (1977): 202. J. A. Lohne, "Essays on Thomas Harriot: I. Billiard Balls and Laws of Collision; II. Ballistic Parabolas; III. A Survey of Harriot's Scientific Writings," *Archive for History of Exact Sciences* 20 (1979): 201. Jon V. Pepper, "Harriot's Manuscript on the Theory of Impacts," *Annals of Science* 33 (1976): 133.

[8] For an extensively argued paper on the general relationship between optics and collision in the 17th century see Russell Smith, "Optical reflection and mechanical rebound: the shift from analogy to axiomatization in the seventeenth century, Part 1," *British Journal for the History of Science* 41 (2008): 1-18, and "Part 2," *British Journal for the History of Science* 41 (2008): 187-207. Harriot's *De reflexione corporum rotundorum* is discussed in Smith, "Optical reflection," 7-18.

[9] Jacquot, "Harriot's Reputation," 180-1; Lohne, "Brahe of Optics," 115. In this letter, Kepler also asked Harriot questions on the topic of refraction and the colors of the rainbow.

his letter to Harriot, the *Astronomiae pars optica* had not touched on the physical nature of light, which might *explain*, rather than merely describe, phenomena such as refraction.

> I was overjoyed, excellent Herriot, when Johannes Eriksen, the bearer of these letters, told me there lived in England a man well versed in all mysteries of nature, who if not impeded, was eager to correspond with me through letters, and who possesses, especially in optics, new and unknown principles, by which both my optical book and all previously published are found to be not only deficient, but even erroneous..."[10]

According to Lohne, Kepler had attempted to draw insights into the nature of light from the collision of bodies. "[Kepler] found, however, that although so many authors had discussed motion, none of them had said anything useful about collisions."[11] Although perhaps unknown to Kepler at the time, Harriot may well have had a physical explanation for refraction of the very kind that Kepler had considered—one based on the collision of bodies. The evidence for Harriot's position is indirect. It comes from the correspondence with Kepler, as well as a manuscript essay, *Synopsis of the Controversie of Atoms*,[12] written by a former student of Harriot, Nathaniel Torporley, after Harriot's death, in which Harriot's views are criticized.

Torporley describes Harriot's explanation of phenomena, such as refraction, as follows. A transparent body is composed of atoms organized in a regular array with empty space between them. Light reflects off one atom and to another and back to another in a zigzag fashion.

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[10] Johannes Kepler, *Joannis Kepleri Astronomi Opera Omnia*, vol. 2 (Frankfurt: Heyder & Zimmer, 1859) 67. Kepler to Harriot, 2 October 1606. “Lecto meo libro ignorare non potes, quibus in quaestionibus a te cupiam erudiri, adeo frequenter ad tui similium provacavi solertiam. Capite I. principiis usus sum theologicis magis quam optics, quae res arguit, me naturam lucis penitus ignorare.” Translation quoted in J. A. Lohne, "Thomas Harriott (1560-1621), The Tycho Brahe of Optics," *Centaurus* 6 (1959): 115. Also see Jean Jacquot, "Thomas Harriot’s Reputation for Impiety," *Notes and Records of the Royal Society* 9 (1952): 180-1.

[11] Lohne, "Essays on Thomas Harriot," 207. Unfortunately, Lohne does not cite any sources wherein Kepler may have expressed this intuition.

[12] Nathaniel Torporley, "A Synopsis of the Controversie of Atoms," Birch MS 4458 ff. 6-8, in Jean Jacquot, "Thomas Harriot's Reputation for Impiety," *Notes and Records of the Royal Society* 9 (1952): 184.

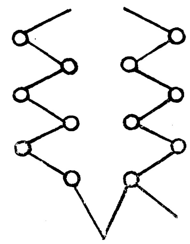

Figure 1. From Torporley's essay, "A Synopsis of the Controversie of Atoms"

Since the human eye cannot directly observe the motion of atoms, nor can it discern distances of only a couple atoms in length, to normal human observation light appears to move in straight lines. When the light moves from one transparent body to another, the regular arrangement of atoms is different, which affects the angles in the reflecting zigzag path of light.[13]

Torporley had been close to Harriot. He was entrusted to edit Harriot's mathematical manuscripts[14] and to publish the results of his studies.[15] Torporley was well qualified for this task, since he had been personally tutored by Harriot, and had also spent time in France as an assistant to the mathematician François Viète in 1591, the very time of the publication of Viète's *Algebra nova*.[16] It was through Torporley that Harriot

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[13] Robert Goulding, "*Chymicorum in morem:* Refraction, Matter Theory, and Secrecy in the Harriot-Kepler Correspondence," in *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England,* ed. by Robert Fox (Burlington: Ashgate Publishing, 2012), 41-6. Also see Jacquot, "Harriot's Reputation," 180.

[14] Robert Hugh Kargon, *Atomism in England from Hariot to Newton* (Oxford: Clarendon Press, 1966) 33.

[15] John W. Shirley, *Thomas Harriot: A Biography* (Oxford: Clarendon Press, 1983) 2. Harriot named Torporley in his will. "I ordayne and Constitut the aforesaid Nathaniell Thorperley first to be Ouerseer of my Mathematicall writinges to be received of my Executors to pervse and order and to separate the Cheife of them from my waste papers, to the end that after hee doth understande them hee may make vse in penninge such doctrine that belonges vnto them for publique vses as it shall be thought Convenient by my Executors and him selfe." Quoted in Shirley, *Biography*, 2. Also see Jacquot, "Harriot's Reputation," 165.

[16] Shirley, *Biography*, 3. Also see Jon V. Pepper, "A letter from Nathaniel Torporley to Thomas Harriot," *British Journal for the History of Science*, 3 (1967): 285-90, for an account of a letter Torporley sent to Harriot, which shows that Torporley and Viète had been friends as early as 1586. A problem Viète passed to Harriot while Torporley was in France can be found among "Harriot's mathematical and scientific papers" held at the British Library, Add. MS 6782 f. 483.

became "so intimately familiar" with Viète's algebra.[17] However, after Harriot's death, Torporley did not publish the mathematical writings. To amend this situation, Walter Warner, another of Harriot's close friends, assembled and published a volume on algebra under the title *Artis Analyticae Praxis* (1631). Much of Harriot's reputation for centuries to come would rest solely on this publication. Recent scholarship by Jacqueline A. Stedall has shown that "the editors selected and reordered Harriot's work in such a way that the *Praxis* often bears little resemblance to the manuscripts, and fails to do full justice to the quality and originality of Harriot's insights."[18] Four centuries prior, Torporley appears to have felt the same way and criticized the editors for the way they presented Harriot's work, going to such lengths as to prepare a title page of a work which would fix these problems called, *The Analytical Corrector of the posthumous scientific writings of Thomas Harriot*.[19] Unlike Stedall in the 21st century, Torporley in the 17th did not publish the correction. Also, unlike Stedall, Torporley himself severely criticized Harriot. On the title page he included a double-edged eulogy of his deceased mentor and associate:

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[17] Jacqueline A. Stedall, "Reconstructing Thomas Harriot's Treatise on Equations." In *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England,* edited by Robert Fox, 53-64 (Burlington: Ashgate Publishing, 2012), 53-4. Also see Jacqueline A. Stedall, *The Great Invention of Algebra: Thomas Harriot's Treatise on Equations* (New York: Oxford University Press, 2003) 3-7, 17-20, for an account of the relationship between Harriot, Torporley, and Viéte.

[18] Stedall, *Great Invention*, 3. Stedall has done extensive archival work to rectify the poor presentation of Harriot's algebra. She writes that "the relevant sheets are now separated and dispersed amongst the surviving manuscripts, but Harriot's pagination, the mathematical content and other contemporary evidence allow them to be reassembled in what appears to be the original sequence. The result is a lucid and self-contained *Treatise on Equations.*" In 2003 Stedall published this "treatise" for the "first time in its original form."

[19] Shirley, *Biography*, 5. Also see Henry Stevens, *Thomas Hariot, the Mathematician, the Philosopher and the Scholar: Developed Chiefly from Dormant Materials, with Notices of His Associates, Including Biographical and Bibliographical Disquisitions Upon the Materials of the History of 'Ould Virginia'* (London: Privately Printed, 1900) 174. Also see Nathaniel Torporley, "Corrector Analyticus, or strictures on the Artis Analyticæ Praxis of
Thomas Harriot," in *A collection of letters illustrative of the progress of science in England, from the reign of Queen Elizabeth to that of Charles the Second*, ed. James Orchard Halliwell-Phillipps (London: R. & J. E. Taylor, 1841), 109-116.

> As an excellent Mathematician, one who very seldom
> As a bold Philosopher, one who occasionally
> As a frail Man, one who notably
>
> erred,[20]

Torporley then went on to announce an outright attack on Harriot's philosophy:

> For the more trustworthy refutation of the pseudo-philosophic atomic theory, revived by him and, outside his other strange notions, deserving of reprehension and anathema.[21]

It is in Torporley's careful assault, *A Synopsis of the Controversie of Atoms*, that we find a reconstruction of Harriot's atomistic explanation of the nature of light. Torporley presented what he took to be Harriot's general maxims,[22] and then concentrated on Harriot's explanation of the "refraction phenomena observed in a hollow sphere of crystal," wherein "refraction consisted in a series of reflexions."[23]

Harriot replied to Kepler on 2 December 1606. He provided Kepler with a table of angles of refraction, which he had derived from experimental investigation.[24] But in the letter he also suggested that dense and transparent bodies only *appear* to be continuous, and, in keeping with Torporley's account, are actually composed of both parts and vacua. Harriot defended this position by referring to a phenomenon from common experience: when a ray hits dense transparent surfaces it appears to be partly reflected and partly refracted.

> [The body] has corporeal parts which resist the rays, and incorporeal parts [vacua] which the rays penetrate. And so refraction is nothing else than an internal reflection, and the part of the rays which received inside, although to the sense it appears straight, is nevertheless composed of many [straight-line segments].[25]

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[20] Stevens, *Hariot the Mathematician, Philosopher, & Scholar*, 174.
[21] Ibid., 174. Stevens reproduced and translated into English the entire title page of the *Corrector Analyticus*.
[22] Jacquot, "Harriot's Reputation," 183.
[23] Ibid., 180.
[24] See Lohne, "Brahe of Optics" for a detailed analysis of Harriot's experimental work on refraction.
[25] *Kepleri Opera omnia* 2:72. Harriot to Kepler, 2 December 1606. Translation quoted in Kargon, *Atomism in England*, 130. Also see Jacquot, "Harriot's Reputation," 180.

Harriot closed his letter to Kepler with language similar to the opening words of *De reflexione corporum rotundorum*, where he explained why it is important to understand the reflection of round bodies.

> I have now led you to the doors of nature's house, wherein lie its mysteries. If you cannot enter because [the doors] are too narrow, then abstract and contract yourself into an atom, and you will enter easily. And when later come out again, tell me what wonders you saw.[26]

1.2 – Mathematics and the mysteries of nature: not pure, mixed, practical, or applied

De reflexione corporum rotundorum is unique. It contains geometric constructions, a complex set of symbolic equations, and a causal account of motion. Although Harriot had worked in many of the domains of mathematics of his time, *De reflexione* does not fit easily into the established traditions of pure mathematics, mixed mathematics, or practical mathematics. It may well be a natural philosophy of light investigated with mathematics, but it is not a work of mathematical optics.

Thomas Harriot (1560-1621) did not explain the distinctions between the various mathematical sciences. His contemporaries, Christoph Clavius (1537-1612) and Francis Bacon (1561-1526), did. Christoph Clavius's commentary on Euclid's *Elements* went through five editions from 1573-1613. It included an account of Proclus's (410-485) philosophy of mathematics, and two taxonomies of the mathematical sciences.[27] According to Proclus's Pythagorean taxonomy, a distinction is drawn between those mathematical sciences that consider *multitude* (*i.e.* discrete quantities), and those that

[26] Ibid. Translation quoted in Kargon, *Atomism in England*, 130.

[27] H. M. Mulder, "Pure, Mixed and Applied Mathematics: The Changing Perception of Mathematics through History," *Nieuw archief voor wiskunde*, 8 (1990): 32. Proclus's writings, specifically his commentaries on Euclid's first book, were only rediscovered in the 16th century. Simon Grynaeus edited the first Greek text of Proclus's *Commentary on Euclid's First book*, which was published in 1533 as an appendix to his edition of Euclid. Francis Barocius published the first Latin translation in 1560.

consider *magnitude* (*i.e.* continuous quantities). Each of these can be studied in two ways: multitude as "simple and subsisting in itself" (*Arithmetica*) or multitude "in relation to other numbers" (*Musica*); and magnitude can be studied as stationary (*Geometria*) or in motion (*Astronomia*).[28]

Pythagorean

Multitude | *Magnitude*

simple	in relation	stationary	in motion
Arithmetica	*Musica*	*Geometria*	*Astronomia*

The other classification provided by Proclus is that of Geminus (fl. 70 BCE). Geminus drew a distinction between *sensibles* and *intelligibles*. The latter are "objects aroused by the soul herself and contemplated in separation from embodied forms."[29] These comprise arithmetic and geometry. He includes the six sciences—*mechanica, astronomia, optica, geodesia, musica,* and *logistica* [*i.e.* calculation]—in the category of sensibles.

Geminian

Intelligibles	Sensibles
arithmetic	*mechanica*
geometry	*astronomia*
	optica
	geodesia
	musica
	logistica

Clavius's preferred distinction was between *pura* (pure) and *mixta* (mixed). The use of the word "mixed" was new to the late 16^{th} and early 17^{th} century, but it would be taken over in many classifications of mathematics. It included not only the "Pythagorean" harmony and astronomy, but the "Geminian" *mechanica, optica, geodesia,* and *logistica*.

[28] Ibid., 30.
[29] Ibid., 30.

By the 18th century the number of mixed mathematics had increased to about twenty.[30] Only in the 19th century would the term (and conception of mathematics) be displaced by "applied mathematics,"[31] in which an abstract mathematical structure is used to model some phenomenon or process in the world.

Harriot's near contemporary, Francis Bacon, explained the meaning of "mixed" mathematics, and described the distinction between it and "pure" mathematics in the context of his larger categorization of knowledge in *Of the Proficience and Advancement of Learning* (1605).

> [T]o pure mathematics belong those sciences which handle Quantity entirely severed from matter and from the axioms of the natural philosophy. These are two Geometry and Arithmetics; . . . Mixed Mathematics has for its subject some axioms and parts of natural philosophy, and consider quantity in so far as it assists to explain, demonstrate, and actuate these.[32]

This notion of "mixed mathematics," understood as the mixing of parts of natural philosophy into the axioms of mathematics, is a much more ancient tradition.[33] It bears a relationship to the Aristotelian notion of "composite science." Geometry investigates "physical lines, but not *qua* physical" – they are abstracted from the physical. Optics and astronomy, on the other hand, investigate "mathematical lines *qua* physical, not *qua* mathematical." In so far as the objects of study of the composite sciences were physical,

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[30] Mulder, "Pure, Mixed and Applied," 32. By the 18th century, the definition of "mixed" mathematics had become fairly well established; for example, according to Chalmer's 1728 *Cyclopaedia*: "Mathematics are divided into pure and abstract; and mix'd. Pure Mathematics consider Quantity abstractedly; and without any relation to Matter: Mix'd Mathematics consider Quantity as subsisting in material Beings, and as continually interwove." Quoted in Mulder, "Pure, Mixed and Applied," 34.

[31] Gary I. Brown, "The Evolution of the Term 'Mixed Mathematics,'" *Journal of the History of Ideas* 52 (1991): 81-102.

[32] Quoted in Brown, "Evolution of the Term," 81-82. Bacon included architecture, "engineery," perspective, and cosmography to his list of mixed mathematics in addition to astronomy and music.

[33] Brown, "Evolution of the Term," 81-82; Mulder, "Pure, Mixed and Applied," 31; Margaret Osler, "New Wine in Old Bottles: Gassendi and the Aristotelian Origin of Early Modern Physics," *Midwest Studies in Philosophy* 26 (2002): 167.

the science was "subordinated" to physics; in so far as the attributes were mathematical and not physical, the science was subordinated to mathematics.[34]

Mixed mathematics is distinct from the modern notion of "applied mathematics." It was also distinct from the 16th and 17th century tradition of "practical mathematics," which included topics such as surveying, cartography, and navigation.[35]

Harriot had done significant work in each of pure, mixed, and practical mathematics. In pure mathematics, for example, he derived and used the "binomial theorem for fractional indices...long before Newton," and rectified the plane equiangular spiral as well as the twisted loxodromic curve on the sphere, which Descartes had thought to be "beyond human knowledge."[36] He extended Viète's work, *De numerosa potestatum resolutione* (*On the numerical solution of equations*), on the structure and solution of, what we would now call, "polynomial equations."[37] Harriot was well versed in practical mathematics as a surveyor, navigator, practical astronomer,[38] and "excellent calculator."[39] He gave instruction in the use of instruments and charts to his patron Sir Walter Ralegh and the captains of Ralegh's expeditions, and took part in transatlantic voyages to North America.[40] Harriot investigated topics in mechanics such as projectile motion.[41] He also

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34 Brown, "Evolution of the Term," 82.
35 E. G. R. Taylor, *The Mathematical Practitioners of Tudor and Stuart England* (New York: Cambridge University Press, 1954).
36 Jon V. Pepper, "Thomas Harriot and the Great Mathematical Tradition." In *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England,* edited by Robert Fox (Burlington: Ashgate Publishing, 2012), 14-17.
37 Stedall, "Reconstructing," 53-7.
38 J. A. Bennett, "Instruments, mathematics, and natural knowledge: Thomas Harriot's place on the map of learning," in *Thomas Harriot: Elizabethan man of Science*, ed. by Robert Fox (Burlington: Ashgate Publishing, 2000), 137-152.
39 Pepper, "Great Mathematical Tradition," 15.
40 Pascal Brioist, "Thomas Harriot and the Mariner's Culture: On Board a Transatlantic Ship in 1585." In *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England,* edited by Robert Fox (Burlington: Ashgate Publishing, 2012), 183-5.
41 Matthias Schemmel, *The English Galileo: Thomas Harriot's Work on Motion as an Example of Preclassical Mechanics*, 2 vols. (Berlin: Springer, 2008).

made significant discoveries in mixed mathematics, specifically mathematical optics. Harriot successfully determined what we now call "Snell's Law" using experiments well before Descartes published his account of the sine law of refraction in the *Dioptrics* (1637), and before Willebrord Snell circulated his results in manuscript form around 1621.[42] He also measured the dispersion of white light with the aid of a prism, determined the radius of the rainbow, and showed that the rays of green and red light have different refrangibilities.[43]

Harriot worked extensively throughout the recognized domains of mathematics, and traversed several traditional intellectual boundaries.[44] Although Harriot was well versed in mathematical optics, and although *De reflexione* may well be connected to his studies of light, it does not easily fit into the category of mixed mathematics. Rather, it appears to be an explanation of the nature of light. If refraction is an "internal reflection" as Harriot claimed in a letter to Kepler, then *De reflexione corporum rotundorum* is a

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[42] John W. Shirley, "An Early Experimental Determination of Snell's Law," *American Journal of Physics* 19 (1951): 507-8. Also see Lohne, "Brahe of Optics," 113-121. Pepper, "Great Mathematical Tradition," 15.

[43] Lohne, "Brahe of Optics," 113-121. Also see J. A. Lohne, "The Fair Fame of Thomas Harriott: Rigaud versus Baron von Zach," *Centaurus* 8 (1963) 69-84. Kargon provides additional references to optics in Harriot's manuscripts in Kargon, *Atomism in England*, 23. Harriot also maked numerous references and notes on the optical works of the 11th century mathematician Alhazen through out the manuscripts. Add. MS 6789 ff. 415-423. "Alhazen" appears in another series of folios: Add. MS 6789 ff. 168, 174-182. *De puncto reflexionis in sphærico convexo* and *De puncto reflexionis in sphærico concava*. The following references to Alhazen are written on the top of the latter series of folios: *Alhazen.* p. 170 lib. 5. n. 73 (Add. MS 6789 f. 174); *Alhazen. part*. 149 (Add. MS 6789 f. 178); *Alhazen*. *part*. 169 (Add. MS 6789 f. 179); *Alhazen* 146 *part* (Add. MS 6789 f. 180); *Alhazen, part* 146 (Add. MS 6789 f. 181); *Alhazen. part.* 146 (Add. MS 6789 f. 182). Also see Stevens, *Hariot the Mathematician, Philosopher, & Scholar*, 179.

[44] J. A. Bennett has called Harriot a "challenging and pioneering figure – a traveler and an interloper on the map of learning." See Bennett, "Instruments, mathematics, and natural knowledge," 151. Stephen Clucas has emphasized a similar point, stressing his many "diverse practices" and the "different faces of Thomas Harriot, and how these faces have changed according to changing conceptions of the knowledge field of his times." See Stephen Clucas, "Thomas Harriot and the field of knowledge in the English Renaissance," in *Thomas Harriot: An Elizabethan Man of Science*, ed. by Robert Rox (Burlington: Ashgate Publishing, 2000), 93, 135. Robert Fox framed the introduction to the 2012 collection of Harriot Lectures in terms of Harriot's "many worlds." See Robert Fox, "The Many Worlds of Thomas Harriot," in in *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England*, ed. by Robert Fox (Burlington: Ashgate Publishing, 2012), 1.

mathematical account of this process. Whether or not Harriot had developed a thorough natural philosophy or was committed to atomism as some have suggested,[45] the mathematical account of collision in *De reflexione*, which Harriot introduced as "lead[ing]...towards the innermost Mystery, or the understanding of Natural Philosophy," was novel.

1.3 – Geometrical constructions, symbolic equations, and numerical calculations

De reflexione corporum rotundorum, Poristica duo was written no later than June 13, 1619, but based on "ancient notes" from several years prior. It was written at the request of Harriot's imprisoned patron, Henry Percy, the ninth Earl of Northumberland who had been locked in the Tower of London since 1606 on charges of treason for his alleged affiliation with the conspirators of the Gunpowder Plot.[46] As it happens, the Earl was set free only days after Harriot died in July 1621.[47] Harriot's previous patron and long time friend, Sir Walter Ralegh, had been executed on the scaffold on 29 October 1618. Harriot was present at Ralegh's last moments alive. According to the notes he took

[45] There has been an on-going disagreement regarding the existance and extent of Harriot's natural philosophy. Although Harriot did not write an explicit treatise on natural philosophy, several historians claim that a coherent natural philosophy uniting atomism, alchemy, and optics is apparent in the manuscripts. For example, see Hilary Gatti, "The natural philosophy of Thomas Harriot," in *Thomas Harriot: Elizabethan man of Science*, ed. by Robert Fox (Burlington: Ashgate Publishing, 2000), 64-92. Also see Goulding, "Chymicorum in morem." Others, notably John Henry, argue that there is no such natural philosophy; Harriot was primarily a mathematician and not interested in natural philosophy. See John Henry, "Harriot and Atomism," in *Mathématiques et connaissance du monde réel avant Galilée*, ed. by Sabine Rommevaux (Montreuil: Omniscience, 2010), 113-54. John Henry, "Thomas Harriot and Atomism: a reappraisal" *History of Science* 20 (1982): 276-296. John Henry, "Why Thomas Harriot was *Not* the English Galileo," in *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England*, ed. by Robert Fox (Burlington: Ashgate Publishing, 2012), 113-138.
[46] G. R. Batho, S. Clucas, and Anna Beer, "The Prison Writings of Sir Walter Raleigh and the Ninth Earl of Northumberland," *The Durham Thomas Harriot Seminar,* Occasional Papper 9 (University of Durham, 1996): 1. Also see Shirley, *Biography*, 327-357.
[47] Gatti, "The natural philosophy of Harriot," 64.

on Ralegh's final speech, his patron "speake iustly & truely."[48] When Harriot sent the treatise on the reflections of round bodies to the Earl, he included a letter, dated 13 June 1619, which indicates some of the practical reasons for writing the treatise:

> S^{r}: when M^{r} Warner & M^{r} Hues were last at Syon, it happened that I was perfectting my auntient ^ notes ~~papers~~ of the doctrin of reflections of bodyes. Unto ^ whom I imported the Magisteryes ~~of~~ thereof, to the end to make your Lord thus aquainted with them as occasion served. And least that some perticulars might be mistaken or forgotten, I thought best since to set them downe in writing. Whereby allso nows at times of leasure when your minde is free from matters of greater waight, you may thinke & consider of them, if you please. Yt had bin verie conuenient I coufe to have written of this doctrine more at longt or particularlie to have set downe the first princeples with such other of elementall propositions, as all doubtes might haue bin c~~leered~~ prevented: but my infirmitie is yet so troublesome, that I am forced, as well that, as other traits to let alone till time of better abilitie. | In the meane time I haue made choyce of two propositions in whose explication you shall find I hope the summe of all... [49]

Some of these "auntient notes of the doctrin of reflections of bodyes" may include folios containing numerical calculations,[50] one of which is reproduced below as figure 3. The above quote also indicates that this is a topic that had been discussed with a circle of scholars, including Warner and Hues, and that the treatise was written based on ideas that were quite old. Nevertheless, the form that the treatise took was not yet to the satisfaction of its author, specifically it does not begin with first principles.

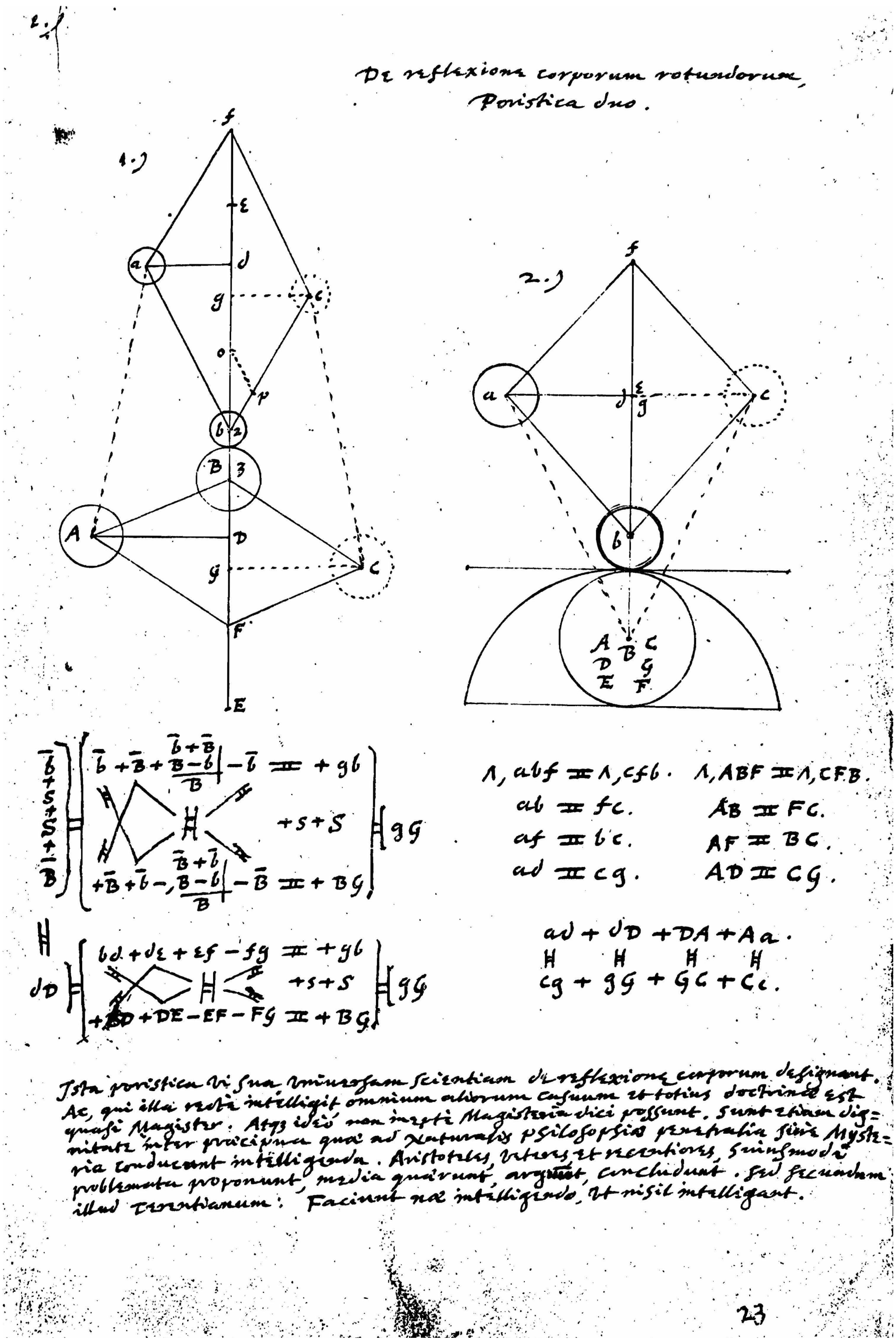
Contained in the manuscript notes and the prepared but unpublished treatise are three kinds of mathematics—the geometric diagram (and construction, written in prose),

[48] Shirley, *Biography*, 446. "[The] half-sheet of wrinkled and battered foolscap which he kept among his miscellaneous papers" on which he kept notes on Ralegh's speech is at the British Library, Add. MS f. 533^{r}. Also see Mark Nicholls, "Last Act? 1618 and the Shaping of Sir Walter Ralegh's Reputation," in *Thomas Harriot and His World: Mathematics, Exploration, and Natural Philosophy in Early Modern England*, ed. by Robert Fox (Burlington: Ashgate Publishing, 2012), 165-182.

[49] Harley MS 6001-2 ff. 21^{r}-22^{v}. Charles Cavendish had copied, in his own hand, many of Harriot's manuscripts, including *De reflexione*. Following the letter, Cavenish wrote: "Syon June 13 . ⊙ 1619 | M^{r}: Harriots letter to my Lo: Northumberland: annexed to his treatise of Reflexions: in ~~th? fus?o~~ lent me to transcribe by S^{r}: Th: Alesburie." Following this, Cavendish has several pages of notes on the treatise. Text transcribed by me from the manuscript held at the British Library.

[50] Add. MS 6786 f. 383, Add. MS 6789 f. 101.

two types of symbolic equations (representing two levels of abstraction), and numerical calculations—all brought to the service of a single phenomenon, the reflection of round bodies.


De reflexione corporum rotundorum,
Poristica duo.

1.)

2.)

$\Lambda, abf = \Lambda, cfb.$ $\Lambda, ABF = \Lambda, CFB.$

$ab = fc.$ $AB = FC.$

$af = bc.$ $AF = BC.$

$ad = cg.$ $AD = CG.$

$ad + dD + DA + Aa.$

$cg + gG + GC + Cc.$

Ista poristica vi sua universam scientiam de reflexione corporum designant. Ac, qui illa recte intelligit omnium aliorum casuum et totius doctrinae est quasi Magister. Atque ideò non inepte Magisteria dici possunt. Sunt etiam dignitate inter praecipua quae ad naturalis philosophiae penetralia sive Mysteria conducunt intelligenda. Aristoteles, veteres et recentiores, huiusmodi problemata proponunt, media quaerunt, arguunt, concludunt. Sed secundum illud Terentianum: Faciunt nae intelligendo, ut nihil intelligant.

23

Figure 2. HMC 241/VIA folio 23r. Image courtesy of the MPIWG Library

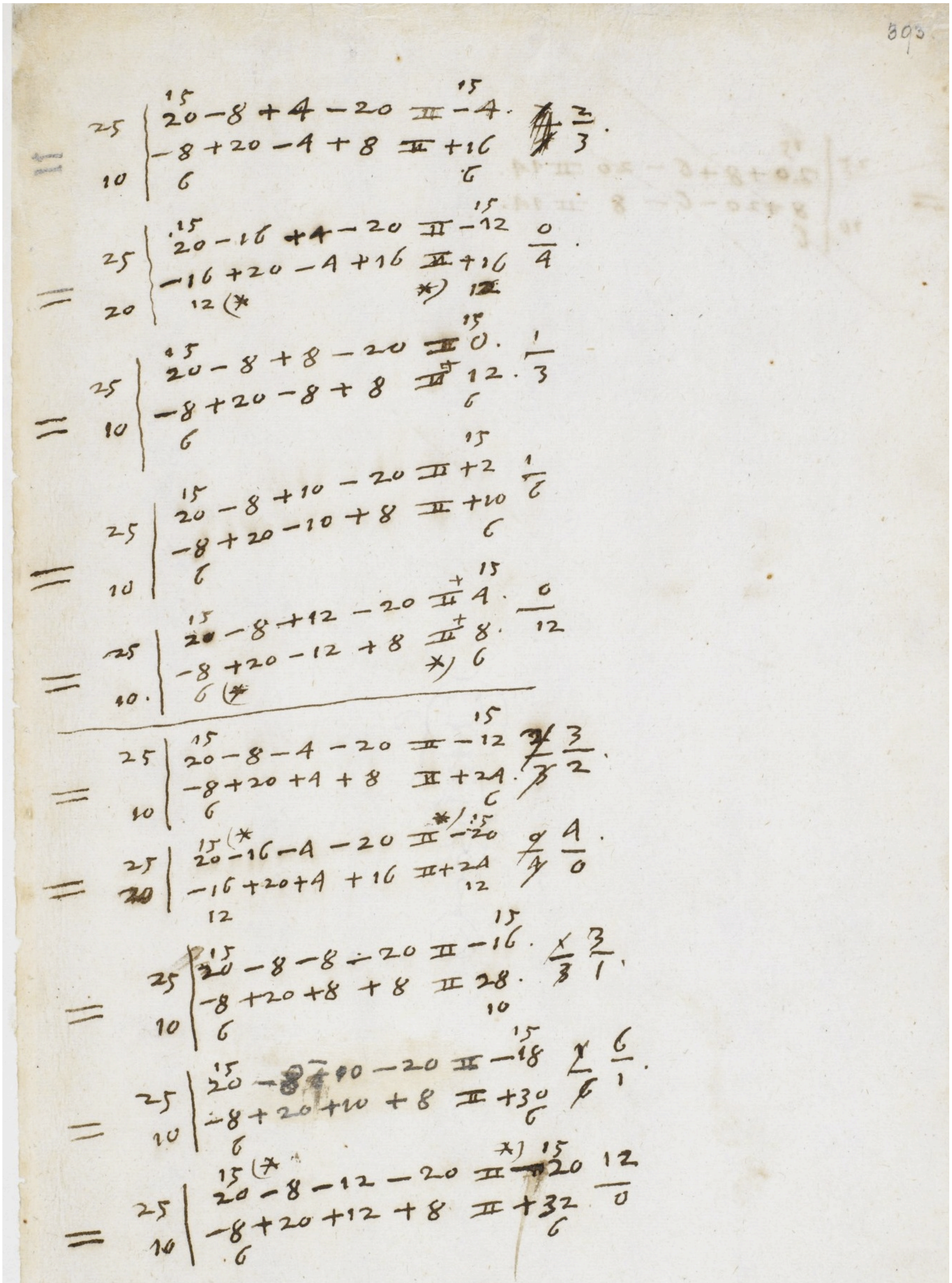


Figure 3. Add. MS 6786 folio 393. Image courtesy of the MPIWG Library and ECHO http://echo.mpiwg-berlin.mpg.de/

Symbolic algebra and the physical interpretation of the negative sign would have an important role in the development of the rules of collision, just as the rules of collision would have an important role in the development of the interpretation of negative numbers, as we will seen throughout this dissertation. Harriot's work on collision is fascinating since it is expressed in part in symbolic algebra and uses what initially appears to be negative numbers. And *De reflexione*, together with Beeckman's independent and simultaneous work in his *Journal*, is among the first mathematical studies of collision. Harriot's *De reflexione* does not appear to have been directly influential on the development of the rules of collision. Whereas Beeckman's work may well have been. For instance, Descartes's early view of collision bears a striking resemblance to that of Beeckman. Nevertheless, Harriot's *De reflexione* is important. In this text we see a possible combination of key mathematical concepts with the concepts of collision, that ultimately did *not* prevail. The use of algebraic equations to express collision in which positive and negative signs indicate the direction of motion, seems obvious in hindsight. It may seem so simple that we risk overlooking their importance. This can be clarified by examining some of the alternative approaches that did not ultimately come to be accepted.

Harriot not only used an algebraic formalism to express the reflection of bodies, but he appears, at least *prima facie*, to use the negative sign to represent certain kinds of speeds. This is remarkable since the legitimacy of negative numbers was disputed at the time and would continue to be debated for years to come.[51] Harriot’s use of the negative

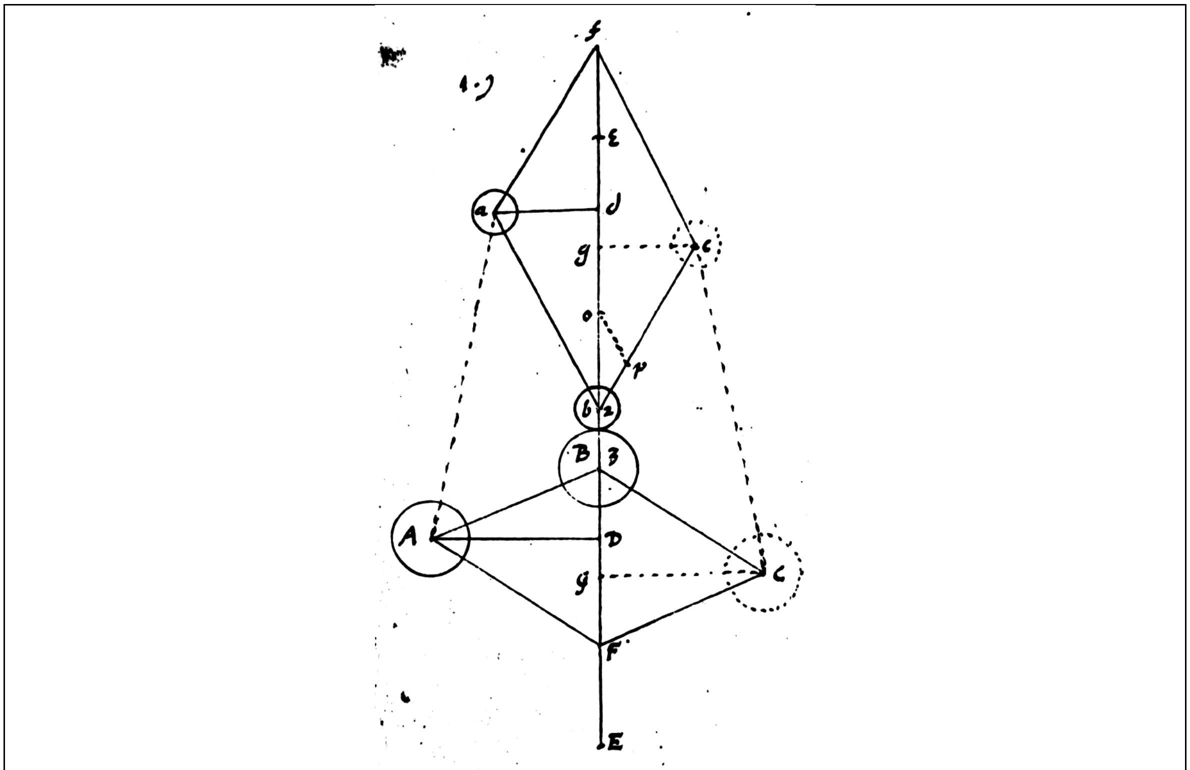
[51] For mathematicians in the 17th century it was difficult to accept that a magnitude could be smaller than nothing. Some algebraists, such as Cardano and Descartes called them "defective," "fictitious," or "false," whereas others simply ignored the negative roots of polynomials. Some, such as Descartes, drew a distinction between "species" marked by a negative sign such as -2A which were deemed acceptable in

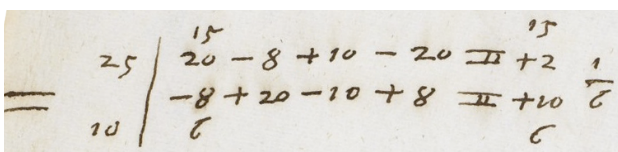
sign, I would like to stress, is quite different from the familiar approach. The geometric construction is fundamental in *De reflexione*. The set of equations is a symbolic expression of the steps of the construction. Positive and negative signs refer to the addition or removal of a line. They do not refer directly to the direction of motion. The equations do, however, provide a means to calculate the motion of bodies after collision, as is exemplified by the arrays of numbers seen in figure 3.

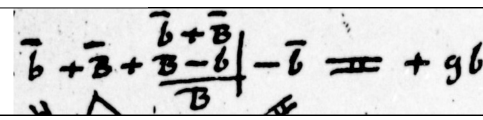
1.3.1 – Negative signs – not contrary direction

There are multiple examples of a negative sign before a number in Harriot's work on collision, which can be seen above in figure 3 as well as below in table 1. The equations from Add MS 6786 f. 393 include four numbers to the left of the equality sign (excluding the numbers above and below the others, and the number to the left of the vertical line). An addition or subtraction sign together with one number follows the equality sign to the right. These numbers correspond to the algebraic terms in the equations from *De reflexione* (on the right of the table). The two numerical equations, one written above the other, correspond to the two algebraic equations, which are linked by a set of zig-zag equality signs.

mathematics, and negative *numbers*, such as -2 which were thought to be impossible. See Helena M. Pycior, *Symbols, Impossible Numbers, and Geometric Entanglements: British Algebra through the Commentaries on Newton's* Universal Arithmetick (New York: Cambridge University Press, 1997), 82-3. Even John Wallis, who would champion the usefulness of negative numbers, acknowledged that they were "impossible." See John Wallis, *A Treatise of Algebra, Both Historical and Practical, Shewing the original, progress, and advancement thereof, from time to time, and by what steps it hath attained to the heighth at which now it is* (London: John Playford for Richard Davis, 1685) 264-5.







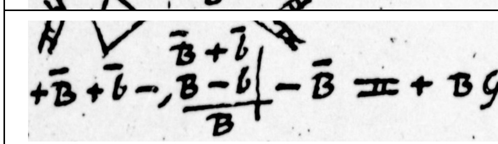


Table 1. Numerical equations from Add MS 6786 f. 393. Diagram, symbolic equations from HMC 241/VIA f. 23r.

"*gb*" in the upper equation and "*BG*" in the lower equation do not refer to the multiplication of the algebraic term "*g*" by "*b*." Rather these refer to the geometric lines *gb* and *BG* in the diagram. The body at *a* moves to *b* where it meets the body which has moved in equal time from *A* to *B*. After they meet, the former body moves to *c* and the latter body moves to *C*. The line *gb* indicates the vertical component of the velocity of the former body *after* the collision. Similarly *BG*, is the vertical component of the velocity of the latter body *after* collision. According to Harriot, the lines on the diagrams actually refer to the *nutus* of a body, rather than "component of velocity." This notion can be translated as "tendency."

The numbers to the left of the line in each numerical equation refer to the magnitudes of the diagonal lines *ab* and *AB*. For now, we can think of these as the initial velocity of the two bodies. The numbers above and below the first terms of the equations refer to the horizontal component of the initial velocity, just as the numbers above and below the terms after the equality sign refer to the horizontal component of the final velocity. Looking back at figure 3, we see that this horizontal component stays the same before and after the collision in every case.

What looks like the fraction 1/6 to the right of the numerical equation in table 1 refers to the ratio of the sizes of the bodies. In this case, the body which moves from *a* to *c* to *b* is to the larger as 1 is to 6.

Just as *gb* is the vertical component of the final velocity (or *nutus*), the algebraic symbol $\overline{b}$ refers to the vertical component of the initial velocity of the smaller body at *b* at the moment of collision. This can be seen if we refer back to figure 2 (the first folio image), and note the identity between *bd* and $\overline{b}$ in the equations with geometric symbols and the equations with algebraic symbols above them. Thus, in the above table of images (table 1) we have an example of what appears to be a negative initial speed[52] of the second larger body, –8.

[52] Harriot does not use the modern conventional distinction of "speed" to refer to a scalar quantity and "velocity" to refer to a vectoral quantity. However, I have used "speed" here instead of "velocity," because I am referring to a specific quantity and because Harriot's use of a negative sign does not necessarily refer to a particularly directed quantity. I have used "velocity" elsewhere, even though Harriot uses the Latin term *celeritas* (swiftness or speed), because in the abstract, Harriot's notion of *celeritas* does include a directional component.

Harriot presents a scenario of collision with the same quantities, except the larger and smaller bodies have been reversed. The above mentioned set of numerical equations is the fourth from the top in figure 3. The set with the bodies reversed is the fourth after the horizontal line. This set can also be found in table 2.

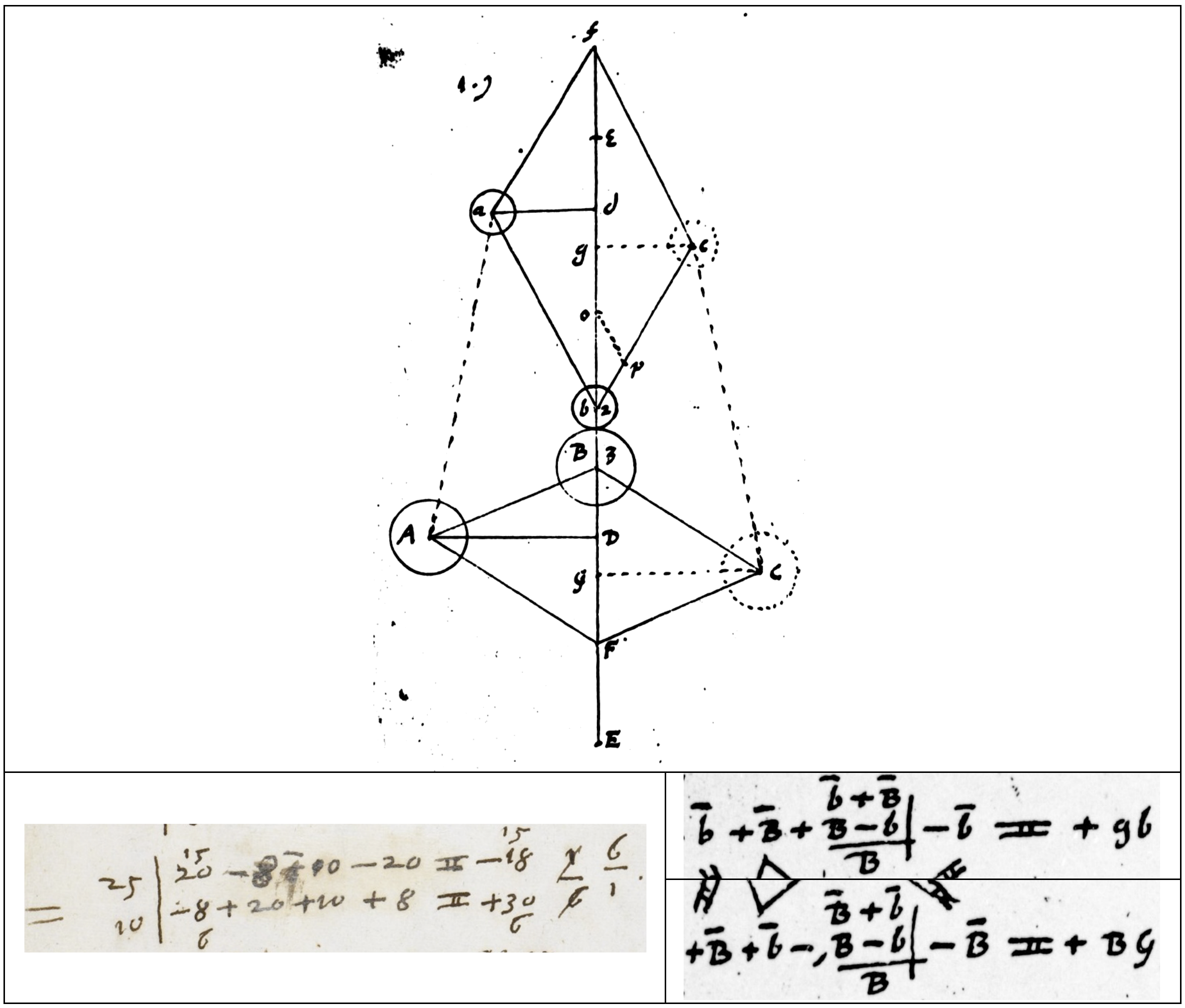


Table 2. Numerical equations from Add MS 6786 f. 393. Diagram, symbolic equations from HMC 241/VIA f. 23r.

Harriot has used the same initial speeds 25 (diagonal), 15 (horizontal), 20 (vertical) for the body moving from *a* to *b* to *c*, and 10 (diagonal), 6 (horizontal), –8 (vertical) for the body moving from *A* to *B* to *C*, just has he had before. However, here the larger and smaller bodies have been reversed. The ratio of the body moving through *abc* to the body moving through *ABC* is 6 to 1, rather than 1 to 6. In this scenario the smaller (rather than the larger body) has an initial vertical speed of –8. We also see that the final speed of the body described in the first equation is negative, –18.

Harriot used negative signs in relation to numbers and symbols in his work on collision. However, the role of the negative sign differs from their modern role as indicating contrary directions of motion. This can be seen clearly with two examples. The bodies described in the above mentioned set of numerical equations in table 2 initially move in the same direction. This is indicated by the second term after the vertical line, as will be explained below. However, the velocity of the larger body is accompanied by a positive sign and the velocity of the smaller body is accompanied by a negative sign. Moreover, after collision, both bodies continue to move in the same direction in this scenario, and yet a negative sign accompanies the larger body and a positive sign accompanies the smaller body after collision. In the second example, when bodies rebound and each change direction after meeting, as is depicted in figure 4 below, all the velocities before and after collision are positive.

1.3.2 – Operations and Constructions

Despite the algebraic formalism in *De reflexione* and despite the numerical calculations based on these equations, Harriot's mathematics of collision is fundamentally geometrical. The two forms of symbolic equations, and the lists of numerical calculations represent a hierarchy of abstraction, that is nevertheless rooted in a geometric construction. This strategy—beginning with a geometrical representation and ending with numerical calculations by means of symbolic algebraic expressions—is consistent with Harriot's general mathematical treatment of other topics in his manuscripts.[53] The steps of the construction dictate the operations in the equations, and the construction provides the meaning of the symbolic and numerical terms.

De reflexione describes how to perform a geometrical construction to determine the "quality and quantity of the motion" after the bodies have "knocked" (*feriunt*) into each other, making a mutual "blow" (*ictus*). The "force" (*vis*) or "active power" of a body (*potentia activa*) acts in the blow, which is created from the meeting of the bodies in motion. This is the cause. The effect, which is what is to be investigated in the study, is the "second motion" caused in each body by the mutual blow. The resulting "second" "apparent motion of the body" (*motus corporis apparens*), is built up from the elemental action of the moving bodies, just as a geometric construction is built up from elemental lines. The motions have various tendencies (*nutus*), which are compounded, subtracted, and transposed by the force of the moving bodies produced from the mutual blow. Rather than a conservation principle or a proportion derived from the mechanical law of the lever, the second apparent motion is produced from three actions, which correspond to three steps in the construction of the diagram.[54]

The central vertical line, seen in figure 4 below, is to be drawn through the center of the two bodies at the point of contact. The new *nutus* acquired by the mutual blow of

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[53] Schemmel, *English Galileo*, 49. "Harriot's mathematical treatment of a mechanical problem usually begins with a geometrical representation of its relevant aspects...and ends with numerical calculations in which definite values for the sought quantities are determined. The algebraic formalism enables Harriot to calculate quantities that do not stand in a relation to the known quantities that is simple enough to be directly read off from the geometrical represenation."

[54] HMC 241 VIA f. 24. "Thus first the lesser body b, whose quantity [of motion] must be separated into three parts, occasioned by as many actions, distinct according to their nature, and as follows." Translation by Lohne, "Essays on Thomas Harriot," 202.

the bodies will be depicted along this line. Next, perpendiculars are to be drawn to the *nutus* line from where the motions of the bodies started. The resulting lines determine the force or active power (*vires siue potentias actiuas*) of the given bodies in the case of the mutual blow.

To understand the first action, and to perform the first part of the construction of the diagram, Harriot asks the reader to consider how the body at capital *B*, if it was equal to that at small *b* and at rest, would react to small *b*, if small *b* struck it with a force as if it moved from *d* to *b*.[55] He claims that the body at *B* would "resist and repel it with a resistance equal to the force received, *viz*, that from *b* to *d*."[56] In other words, if $B = b$, and if *B* is initially at rest, then *B* would repel *b* giving it a new *nutus*, which happens to be the same magnitude as *db* (the component of *b*'s original motion), but directed in the opposite manner. *bd* on the diagram is thus the first part of the second apparent motion.[57] We should note that this line segment, *bd*, is found in the bottom set of equations, and its counterpart in algebraic notation, $\overline{b}$, is in the upper set of equations.

[55] In the text Harriot is not consistent in his naming conventions. At times the name *B* refers to a point on the diagram, at other times *B* designates the body at point *B*. The phrase "body B" is likely a short hand for his other phrase "the body at B." Either the names of the bodies change after equal durations of time *a, b, c* and *A, B, C* respectively, or the bodies are unnamed and occupy differently named positions in equal times. The latter interpretation seems more plausible than the former. However, when Harriot switches to his two forms of symbolic equations (one of which uses conventions from geometric diagrams such as two-letter names for lines, and the other which uses algebraic symbols such as single letters with overlines to indicate lines) the letter *B* as well as the letter *b* refer directly to the body at the point. After Harriot's death, Charles Cavendish had access to many of Harriot's papers from 1621 through 1654. In his study and attempt at systematizing Harriot's work on the reflection of round bodies, Cavendish used another system of notation altogether, which involves superscripts. For example $bd = 1^a$, $de = 2^a$, and the "excess" $ef = 3^a$, where *a* refers to the smaller body. See Harley MS. 6002 ff. $21^r – 22^v$

[56] HMC 241 VIA f.25 Translation by Lohne, "Essays on Thomas Harriot," 202.

[57] It is not insignificant that the initial *nutus* of the body in motion is designated *db*. The *nutus* from the first action—the *nutus* from the other body—is designated *bd*. This is a convinient convention for indicating the direction of a tendency (*nutus*) on Harriot's diagrams.

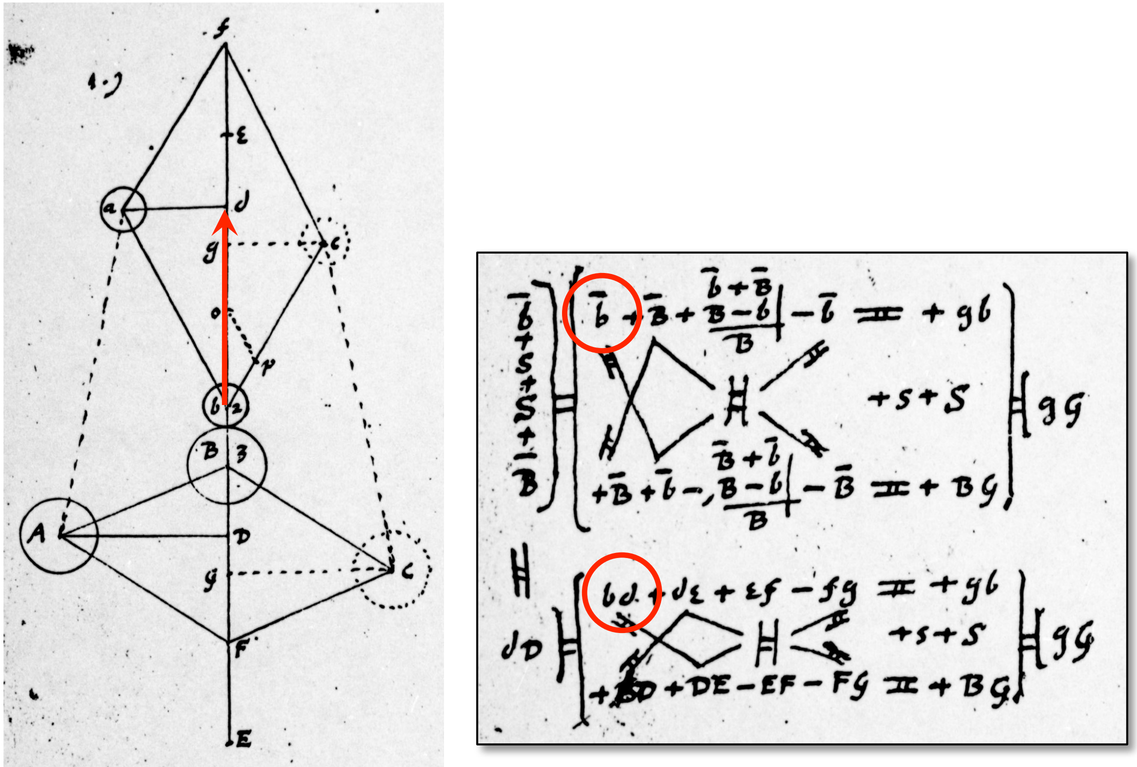


Figure 4. Annotated from HMC 241/VIA folio 23r.

But it is not the case that the body at capital *B* is at rest. So, still assuming that the two bodies are equal, the motion of the body now at capital *B* is taken into account for the second action. A motion equal to that of the vertical component of the motion (the *nutus*) of the body at *B* is added to the *nutus bd*. On the diagram one constructs a line equal in magnitude to *DB*, signified as *de*. Thus, *de* is the second part of the second apparent motion. We also find this step of the construction explicitly in the two sets of equations.

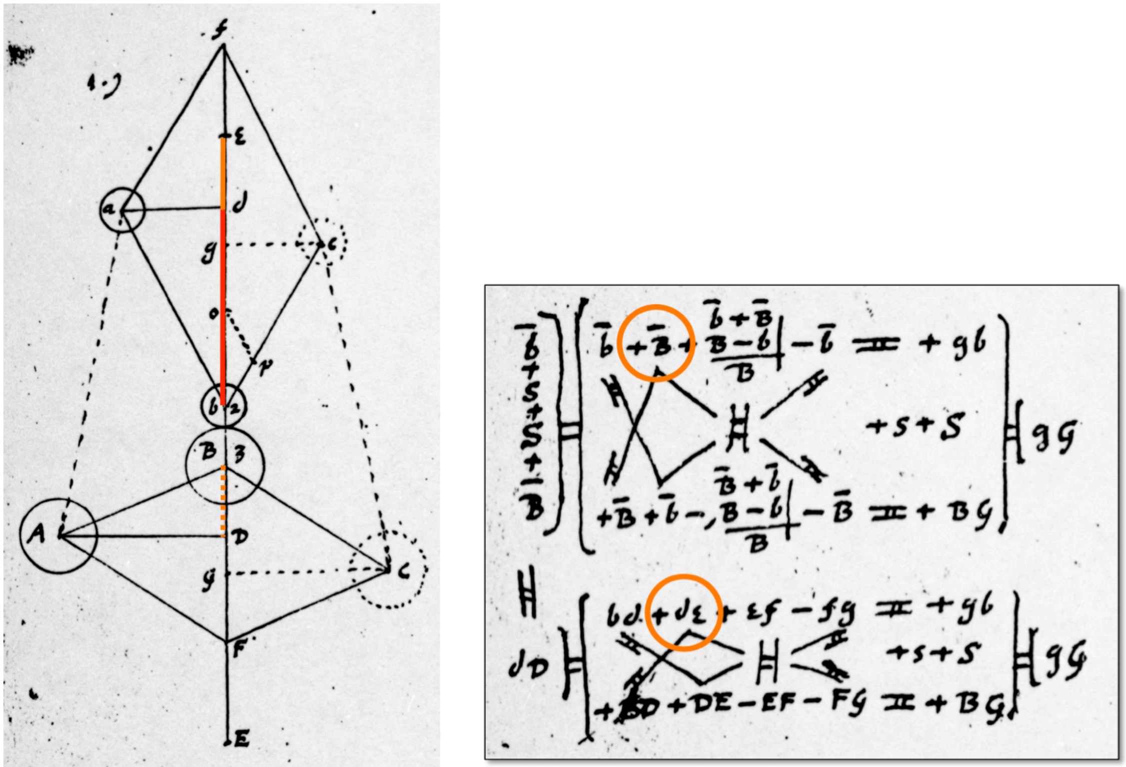


Figure 5. Annotated from HMC 241/VIA folio 23r.

It may not actually be the case that capital *B* is equal to small *b*. If it is larger, then it would produce an effect larger than *be* (the sum of the two parts of the motion already described). And if smaller, it would produce a smaller effect. So, some additional quantity should be included. This additional *nutus*, signified as *ef*, is to *be* as the extent to which body *B* exceeds *b* is to body *B*. In other words,

$$B : B - b :: be : ef$$

Although explicitly left unstated in Harriot's text, we see from the upper equations that Harriot has used the method of 4th entry,[58] to transform the proportion into

[58] Schemmel, *English Galileo*, 49. Schemmel discusses Harriot's use of this method in his section on Harriot's algebra.

an equation and has solved it for *ef*.[59] In the bottom set of equations we see that the third term, the correction value, *ef*, has been added.

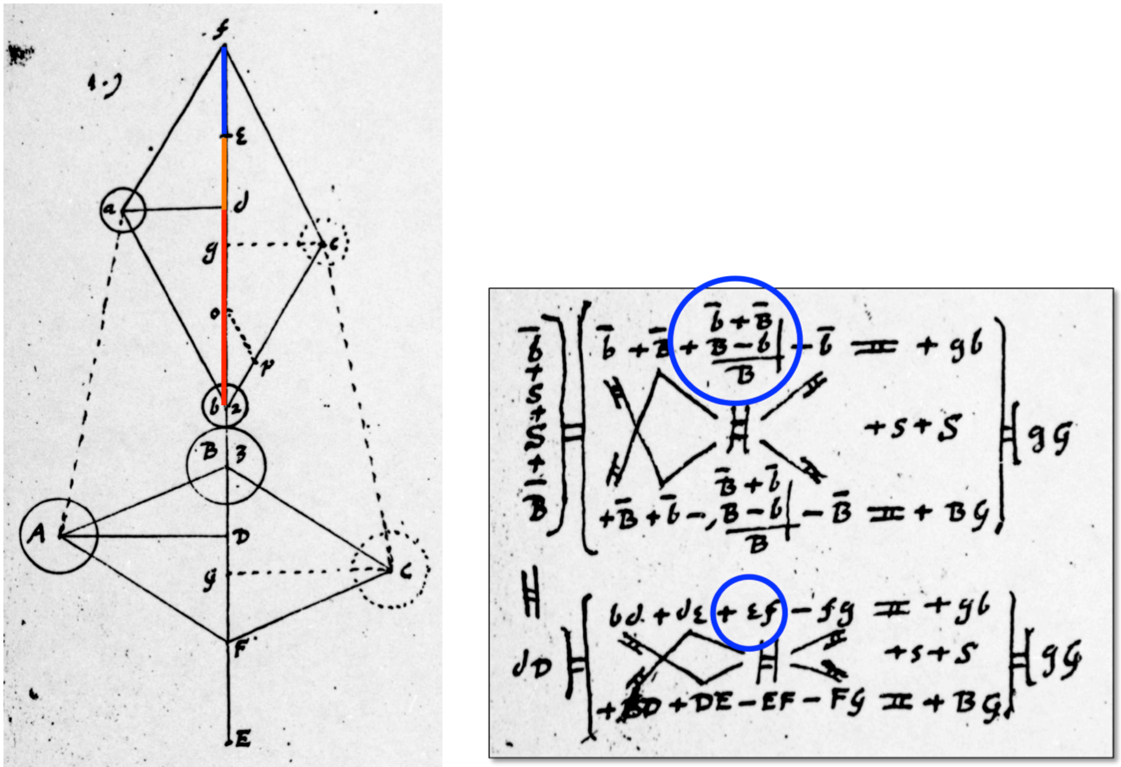


Figure 6. Annotated from HMC 241/VIA folio 23r.

The three lines have been drawn on the diagram, but the end of the line is no where near the smaller body, which is at point *b*. Harriot writes that the smaller body would have continued to move the same space in the same time if it had not hit anything—on the diagram, the line *ab* would be extended by a length equal to itself. However, there is the additional *nutus* (the three actions) due to the mutual blow. On the diagram three lines are added, *bd*, *de*, and *ef,* which correspond to these three actions (the additional *nutus*). The *ictus* does not annul the original *nutus* of body *b*. (The original tendency of body *b*, recall, is *ab*; this is the tendency of its original motion). Rather, the

[59] $B : B - b :: be : ef$ is transformed into the equation, $\frac{B}{B-b} = \frac{\bar{b}+\bar{B}}{ef}$, where $be = \bar{b} + \bar{B}$. This is then solved: $ef = \frac{(\bar{b}+\bar{B})(B-b)}{B}$, which corresponds to the third term in Harriot's upper set of equations.

original *nutus* is deviated by the *ictus*. On the diagram, the line *fc* is drawn parallel to *ab*, but at the end of line *ef* rather than the end of line *de*, which is where it would be drawn had the two bodies never met. This essentially removes the line segment *fg* from the central line of nutus, and the apparent second motion of the smaller body, can be constructed from *b* to *c*. Harriot argues that the three additional tendencies (*bd, de, ef*) and the original tendency of the body (*ab*, now deviated to *fc*) compose the apparent second motion of the smaller body, which on the diagram is the construction of the line *bc*. Thus in the equations we see *fg*, which is equal in magnitude to $\bar{b}$, is subtracted. In order to produce *gb*.

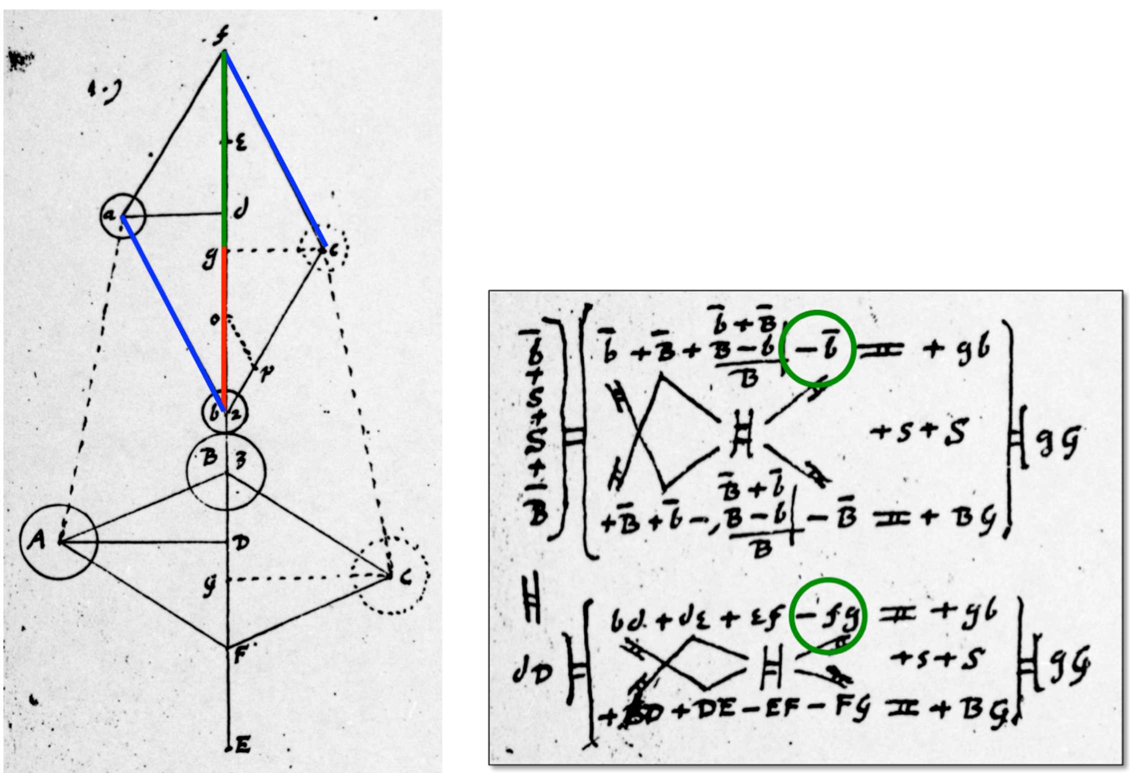


Figure 7. Annotated from HMC 241/VIA folio 23r.

This same construction is repeated for the larger body that moves from *A* to *B* to *C*. First assume that the body it meets is its same size and at rest. This constructs line *BD*. Then take into consideration that the other body, *b*, is moving. This constructs line *DE*.

And third, modify the line thus far constructed according to the correction value. Since the other body, *b*, is *smaller* than *B*, it would produce an effect smaller than *BE.* Some quantity should be removed. This quantity is signified as *EF.* Harriot's correction term, *EF*, has been criticized by several historians. This will be discussed more below in section 1.3.3.

The *operations* in the equations correspond to the steps of the geometric *construction*. The equations refer directly to the lines of the diagram, and only secondarily to "physical quantities" such as the *nutus* themselves. This can most clearly be seen by the presence of the terms *s* and *S*. There are many equalities indicated in Harriot's symbolic notation. For instance the upper bracketed equations are set equal to the lower bracket of equations. I have taken this link to indicate a definition of terms, for instance, $ef = \frac{(\bar{b}+\bar{B})(B-b)}{B}$. Similarly, the geometric line *bd* is $\bar{b}$. The set of upper equations is a higher level in a hierarchy of abstraction. Nevertheless, even these equations are rooted in the diagram. The lower set of equations, as a whole, states that *dD* is equal to *gG*. It is tempting to interpret this as statement that the total speed prior to the meeting of bodies (*dD*) is equal to the total speed after the meeting of the bodies (*gG*). However, strictly speaking, the total speed prior to the meeting is indicated by the sum of speed $\bar{b}$ and speed $\bar{B}$. However, the geometric notation, *dD*, actually refers to the *line*. Since $\bar{b}$ refers to line *bd* and $\bar{B}$ refers to line *BD*, strictly speaking, *dD* is not equal to the total speed prior to the meeting. There is the space that the bodies themselves occupy on the diagram. *S* refers to the radius of the body at *B* and *s* refers to the radius of the body at *b*. Even though this conflates different kinds of quantities, Harriot still includes the half width of each body so that the line corresponds to the algebraic symbols and vice versa, as can be seen in the vertical equation:

$$dD = \bar{b} + s + S + \bar{B}$$

The symbols in the central brackets can also be understood as a single equation. Note that in both sets of brackets the first term of the lower equation is preceded by an addition sign. When all the terms are added and subtracted on the left side of the equation, it amounts to the line segments *gb* and *BG*. The combination of these two line segments is *nearly* equivalent to the line *gG*, which is the line indicated to the right of the brackets and equal sign. However, it is only nearly equivalent to *gG.* What is missing, geometrically, is again the space that the bodies themselves occupy. This is resolved by adding *s*, the radius of the smaller body, and *S*, the radius of the larger body. These terms are included in both the geometrical symbolic equations as well as the algebraic symbolic equations. The addition of the *s*'s appears to be done purely for the sake of maintaining a strict correspondence to the diagrams. The line segments, such as *gb* or *BG*, designate the physical quantities of the *nutus* (tendencies). *S*, on the other hand, designates the length of the radius of the larger body. The difference in the kind of quantity—tendency on the one hand and length of the radius of body on the other hand—is collapsed by the diagrammatic representation. Because of the constraints of the drawing itself, both kinds of quantities are represented as a length. In order for the equation to correspond to the diagram it too must include the radius of the body. Thus equations do not directly represent the quantities such as the total *nutus* before the meeting and the total *nutus* after the meeting. If they did, the radius of the bodies would be irrelevant. Instead, the equations refer to the geometric diagram.

Perhaps Harriot realized the awkwardness of these values and thus did not include them in the proper equations themselves, relegating them to an "in-between" status. They appear in-between the two equations in each set.

Despite the awkwardness of the S's, the equations and diagrams economically contain a great amount of information from Harriot's porisms. For example, not only are the upper and lower bracketed equations equated, as well as the equations within the brackets, the terms are also linked by zigzag equalities. *bd* is equal to *DE* which appears to be equal to *fg*. *bd* is the *nutus* of the body *b* from the first action; it was created by the active power of the body at *B* taken to be at rest; the *nutus* produced was equal in magnitude to the initial speed of *bd* (if the body had moved along the line of *nutus*) but in the opposite direction. This is set equal to *DE*. *DE*, if you recall, is the *nutus* produced in the second action on the body at *B*; it is created by adding a quantity equal to the magnitude of the initial motion of the first body *b*, which is *bd*. And *fg* is the amount subtracted off the line of *nutus* once the original motion has been deviated to the end of the constructed line of *nutus* due to the mutual *ictus* made by the meeting of the bodies. This original motion had a vertical nutus equal to *bd.*[60] The symbolic expressions, with their zigzag equalities contain the information described in this paragraph and more. Whereas a verbal description, such as this very paragraph, is difficult to follow, the symbolic expression is elegant, simple, and intelligible.

[60] In other words, the *nutus* from the first action on the smaller body is the same magnitude as the second action on the larger body, which is also the same magnitude as what is removed due to the original nutus of the smaller body's motion. The smaller body receives a *nutus* from the larger body which is equal to the original nutus of the smaller body, but in the opposite direction. This is the first action with respect to the smaller body. The motion of the smaller body imparts a *nutus* on the larger with a magnitude equal to the original *nutus* of the smaller body. This is the second action with respect to the larger body. Since the original nutus of the smaller body is not annulled, but deviated by the mutual *ictus* made by the meeting of the bodies, this *nutus*, which is subtracted off, is equal in magnitude to itself and to the *nutus* of the second action with respect to the second body.

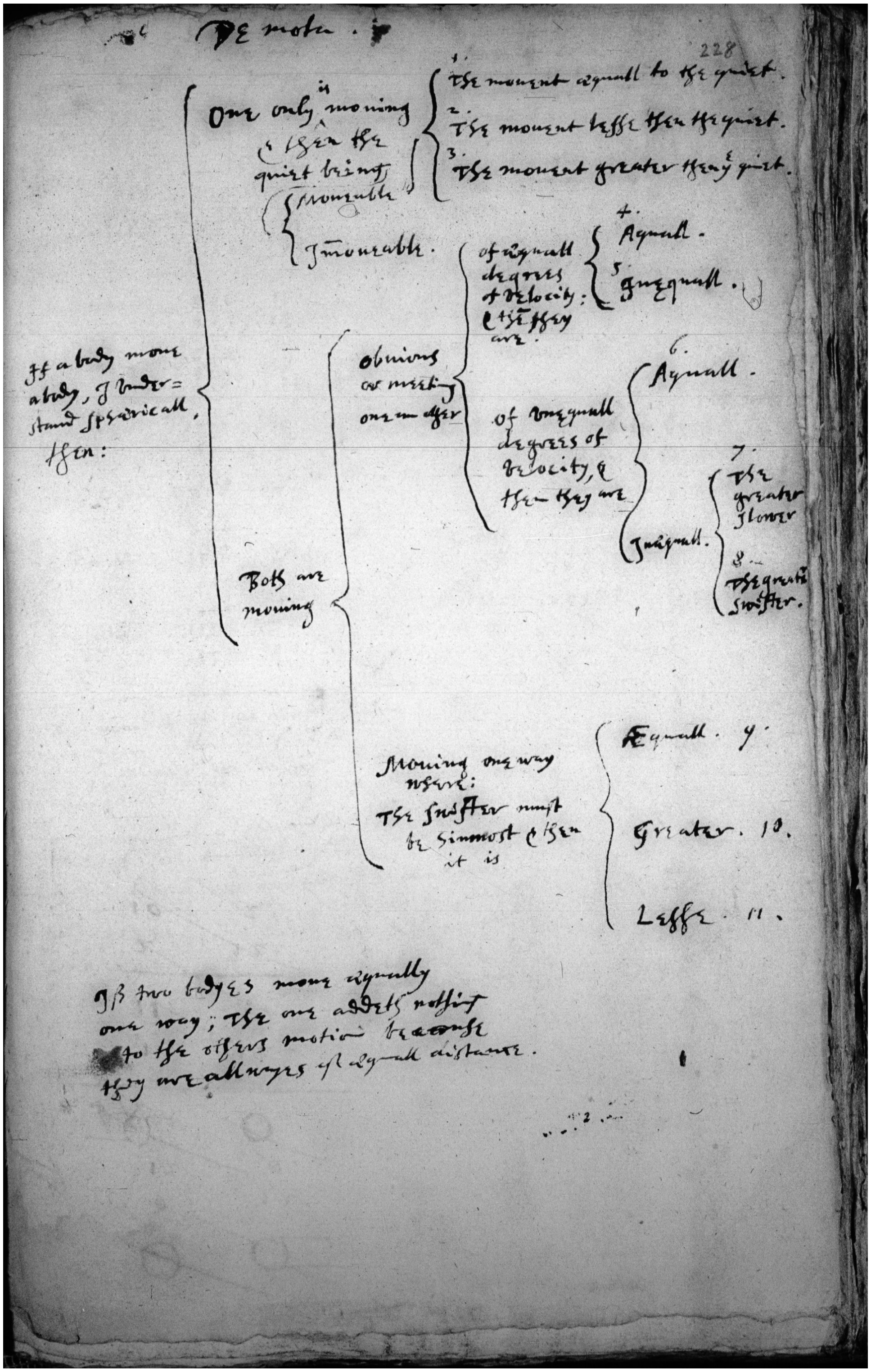
All the magnitudes are non-negative, since they correspond to line segments. The negative signs, such as that in front of *fg*, serve the purpose of *operations.* They do not indicate the "quality" of the quantity, *e.g.* whether it is a negative quantity or a positive quantity. Recall from section 3.1.1 that if a body continues to move in the same direction after it meets another body, then the sign of the number or symbol representing its motion changes from positive to negative. From working through the construction, we know that the *nutus* (for the second motion of the smaller body) are constructed above the point of contact; and the *nutus* (for the second motion of the larger body) are constructed below. We could imagine a line separating these two domains. If the first body continues to move in the same direction after collision, then it will have meant that enough quantities have been subtracted off the line of construction that the entire line has been removed and a line segment extends to the other side of the point of contact in the construction. Since the removal, or subtraction, of a line segment is indicated with a negative sign, this line is numerically presented with a negative symbol.

Also, recall that if the second body initially moves in the *same* direction as the first, it appears to have a negative speed. The value in question is the first term of the equation, which results from the first action. Technically it is not the initial velocity of the body at all. Rather it is the *nutus* imparted on the body in question by the other body, if we assume the other body is at rest and equal in size. Thus a body moving from *D* to *B* would acquire a *nutus* from body *b* equal in magnitude, but in the opposite direction *BD*. If the larger second body moves in the same direction as the smaller first body, when the two bodies meet, the vertical nutus *DB* of the larger body is drawn *above* the imagined line. It is a quantity subtracted off the line of construction for the apparent motion of the larger body.

1.3.3 – Numerical Calculations and Symmetry

In addition to the diagram, the set of geometric symbolic equations, and algebraic symbolic equations, which are on the first folio of *De reflexione*, Harriot also has lists of numerical equations, such as those in figure 3 above. The latter equations appear to have been used to calculate and compare various possible outcomes of colliding bodies. They were likely instances of "mathematical experimentation" with his established structure, possibly as an investigation of symmetry. Nevertheless, a facet of Harriot's equations, namely the term corresponding to the "third action," is inconsistent with principles of symmetry, but not for the reasons that several commentators of Harriot's work have suggested.

The list of numerical equations in figure 3 is not representative of *all* possible combinations of differently sized bodies with different speeds. However, charting out all possible combinations was of interest to Harriot. The folio in figure 8 depicts a taxonomy of eleven different scenarios in which two bodies could meet, *e.g.* both bodies move in opposite directions or the same direction, the speeds are different or equal, one body is at rest, the bodies are of different or equal size.



De motu .

228

If a body move a body, I under= stand sphaericall, then:

One only is moving & then the quiet being
Moveable
1. The movent equall to the quiet.
2. The movent lesse then the quiet.
3. The movent greater then the quiet.
Immoveable.

Both are moving
obvious or meeting one an other
of equall degrees of velocity: & then they are
4. Æquall.
5. Inequall.
of unequall degrees of velocity, & then they are
6. Æquall.
Inequall.
7. The greater slower
8. The greater swifter.
Moving one way where: The swifter must be sinmost & then it is
Æquall. 9.
Greater. 10.
Lesse. 11.

If two bodyes move equally one way; the one addeth nothing to the others motion because they are allwayes of equall distance.

Figure 8. Add MSS 6788 f. 228, courtesy of MPIWG Library

In the 17th century, Sir Charles Cavendish appears to have used this taxonomy as a guide in his notes on Harriot's *De reflexione*.[61] Martin Kalmar, in the 20th century, has also used this taxonomy as a guide in his reconstruction of Harriot's theory of collision, which attempts to uncover its basic principles.[62]

The list of numerical equations, on the other hand, depicts a series of fairly redundant kinds of collisions. Most are scenarios in which both bodies initially move in the same direction. The striking feature of the numerical equations in figure 3 is that the five sets of equations above the horizontal line closely correspond to the five sets of equations below the horizontal line. The various initial tendencies (*nutus*) are the same between the corresponding equations, but Harriot has reversed only the relative sizes of the bodies. The ratio of the bodies in the first set is 2/3 whereas the ratio of the bodies in the first set after the horizontal line is 3/2, just as the second sets are 0/4 and 4/0, the third sets are 1/3 and 3/1, the fourth sets are 1/6 and 6/1, and the fifth sets are 0/12 and 12/0. Another striking feature is that the second and fifth sets of equations (both above and below the line) include a ratio between the bodies in which one term of the ratio is zero. These equations are marked by a double set of asterisks in parentheses. What Harriot's intentions were with these equations is not entirely clear. Nevertheless the sets of equations are suggestive of an investigation into principles of symmetry.

Historians have criticized Harriot's *De reflexione* for failing to properly incorporate principles of symmetry into its equations. The term corresponding to the "third action," described above as the "correction term," is the primary target. Some of the criticisms of this term are a product of the historians' reconstructions of Harriot's

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[61] Harley MS 6001-2 ff. 21^r-22^v.
[62] Kalmar, "Hariot's *De Reflexione*," 206.

theory of collision into modern algebraic equations. With a proper understanding of Harriot's own mathematics, these criticisms are irrelevant. However, even with a historicist understanding of the mathematics in *De reflexione* the third term, particularly in the second of the equations in the sets, is problematic.

Modern textbook presentations of collision use two conservation principles, expressed by two algebraic equations with two variables: the conservation of momentum and the conservation of energy.

$$m_1 v_{1i} + m_2 v_{2i} = m_1 v_{1f} + m_2 v_{2f}$$

$$\tfrac{1}{2} m_1 {v_{1i}}^2 + \tfrac{1}{2} m_2 {v_{2i}}^2 = \tfrac{1}{2} m_1 {v_{1f}}^2 + \tfrac{1}{2} m_2 {v_{2f}}^2$$

In order to predict the motions of the bodies after collision the two equations are solved simultaneously to determine the two unknown values for the final velocities. Despite the significant differences in the concepts used by Harriot and modern textbooks, there is a similarity in form between Harriot's equations and those derived by the conservation of momentum and energy.

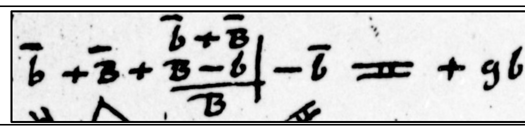
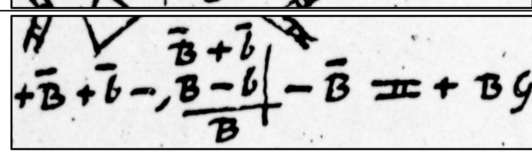
$v_{1f} = v_{2i} + \dfrac{(m_2 - m_1)(v_{2i} - v_{1i})}{m_1 + m_2}$	
$v_{2f} = v_{1i} + \dfrac{(m_1 - m_2)(v_{1i} - v_{2i})}{m_2 + m_1}$	

Table 3

Harriot's equations have four terms on one side of the equation. Two of those terms are the same, the first and fourth, which are added and then subtracted. In the upper equation this is $\overline{b} - \overline{b}$. In this lower equation this is $+\overline{B} - \overline{B}$. Despite the simplicity and redundancy of this operation, it is included in every set of numerical calculations in the list of equations in figure 3. This is another indication that the equations correspond directly to the construction of the diagram. Once this term is "cancelled" from Harriot's equations, what remains is an equation with terms similar to the initial speed of the second body added to a polynomial containing the difference of the initial speeds of the bodies, multiplied by the difference of the bodies, divided by the body. All this is equal to a term similar to the final speed of the first body.

Lohne, Pepper, and Kalmar have claimed that Harriot's correction term should have included the size of *both* bodies added in the denominator. According to their analysis, Harriot's term does not have the required "symmetry properties." [63] These authors have also reconstructed Harriot's theory of collision in modern mathematics. Lohne, for example, explicitly wrote: "I shall not use Harriot's "geometrical methods to introduce the formulæ of collision; rather, I shall use the now well known principles of conservation of "quantity of motion" and "kinetic energy" (force vive)."[64] Lohne went on to derive a reconstructed version of Harriot's equations from these "well known principles

[63] Lohne, "Essays on Thomas Harriot," 195, 196. "In HARRIOT'S correction term [the term corresponding to the third action] we note that the denominator *B* disturbs the symmetry of his equations. ... Did HARRIOT fail to notice that reciprocity in action ought to be described by analogous reciprocity in his formulæ? Or did he not wholly understand that in principle (and in mathematical formulæ) the two bodies should be interchangeable? ... In his coefficient to this relative velocity he fails to see that the magnitudes of *both* balls should be represented in the denominator." Also see Pepper, "Harriot on the Theory of Impacts," 135, 140. "To the modern view the determination of *ef* and *EF* is unsatisfactory. I do not mean merely that the result does not accord with later views, but that it is unsymmetric between the two spheres. ... [F]or example in the case of the smaller sphere, its component of velocity along the line of centres is *bg*, where $bg = df + ef = BD + k(bd+BD)$ and k is the factor which enables the relative masses to be taken into account. In an obvious notation, Harriot explicitly uses $k = \frac{m_2 - m_1}{m_2}$ for the smaller sphere, and $-k$ for the larger, which k lacks the necessary properties. The alternative expression $\frac{m_2 - m_1}{m_1 + m_2}$ does possess the required symmetry and antisymmetry properties, and it is remarkable, to say the least, that Harriot, who as an algebraist knew as much about symmetry as any contemporary, and who in his work on centres of gravity understood mean points, should have produced the fraction ½ rather than 1/5. Perhaps the only safe moral to draw from the situation is that constant cares should be taken against coming to believe that problems which may later seem easy really are easy, even to those of great talent who produce the earlier investigations. Where new or only partly formulated ideas are concerned, nothing is easy." Also see Kalmar, "Hariot's *De Reflexione*," 215-17.

[64] Lohne, "Essays on Thomas Harriot," 191.

of conservation." These authors' suggestions that the correction term could be improved if Harriot had added both bodies $b + B$ rather than merely B, may well be due to their reliance on modern equations such as those in table 3. There is no reason why Harriot would have included the sum $b + B$ in the denominator. The top equation describes the added *nutus* body b receives from its interaction with body B. The third term takes into account that body B is larger than body b. Since B is larger than b, the nutus imparted to b will be proportionally larger than *be* (the nutus imparted to b if b and B were the same size). This extra amount (*ef*) is to the *nutus* (*be*) as the amount the larger body exceeds the smaller ($B-b$) is to the larger body (B). In other words, $ef : be :: B - b : B$. There is no apparent reason for the proportion to be $ef : be :: B - b : B + b$.

The second equation describes the added *nutus* body B receives from its interaction with body b. The third term takes into account that body b is smaller than body B. Since b is *smaller* than B, the *nutus* imparted to B will be proportionally *smaller* than *BE* (the *nutus* imparted to B if B and b were the same size). Thus some quantity, *EF*, should be subtracted from *BE*. Harriot uses the same quantity for *ef* and *EF*. In the second equation the quantity is subtracted, rather than added as it was in the top equation. That Harriot made them equal is made explicit on the first folio of *De reflexione:* a vertical equality symbol extends across the two equations binding together *ef* and *EF*. Moreover, the corresponding third terms for both equations in each set are equal in every numerical example in figure 3. That they are equal is problematic. To be consistent with Harriot's reasoning regarding the third term, *EF* should not be equal to *ef*. Since the *nutus* imparted to B will be proportionally smaller than *BE*, some amount, *EF*, should be subtracted from *BE*. This amount (*EF*) is to the *nutus* (*BE*) as the amount the smaller body falls short of the larger ($B-b$) is to the smaller body (b). In other words, $ef : be :: B - b : b$.

* * *

Thomas Harriot claimed that one should investigate the "reflection of round bodies" because it leads to the "innermost Mystery, or the understanding of Natural Philosophy." This "mystery" was likely the underlying nature of light. If light is to be understood by the collision of bodies, Harriot's manuscript, *De reflexione corporum rotundorum*, presents the mathematics of the causal account of collision. It does so with a geometric construction and symbolic equations. Elsewhere in Harriot's manuscripts he presents lists of numerical equations, based on these sets of symbolic equations, which were likely used to "mathematically experiment" with the structure he had developed. Although Harriot made significant contributions in pure, mixed, and practical mathematics, *De reflexione* does not easily fit into any of these traditional categories. It is not a work in mathematical optics, but it may well be a natural philosophy of light investigated with mathematics.

Harriot's equations appear to use negative numbers. However, on closer inspection, we see that the negative sign does not directly indicate the direction of motion. Rather, Harriot's equations should be understood as a symbolic expression of the steps of a construction. The positive and negative signs indicate the operation of adding and removing lines from the diagram. They do not refer directly to the direction of motion. Nevertheless, the equations do provide a means to calculate the motion of bodies after collision.

Three lines of the construction corresponds to three actions involved in the collision. The construction begins with the simple conceptual scenario as if the two bodies were of equal size, one in motion and one at rest. It is elaborated to include the motion of the other body, and elaborated again to include the relative sizes of the two bodies. With each action, an additional step of the geometrical construction is included. The steps of the construction are abbreviated first by geometrical symbols (such as *bd* and *de*) and arithmetical operations for the steps of the construction. This abbreviated construction is then further abstracted to algebraic symbols (such as $\bar{b}$ and $\bar{B}$) and operators, but still linked together with the former in a network of equality signs. The symbolic term corresponding to the third action has been criticized by historians for conflicting with symmetry principles. Although the term for the third action is problematic, the common criticism—that the denominator ought to include both b and B—seems to be informed by a reconstruction of Harriot's work into modern equations, and a comparison with the equations for the conservation of momentum and energy. This is not a criticism of Harriot's ideas, but a criticism of a mathematical fiction inspired by Harriot's ideas. Concepts from symbolic algebra are key to the development of theories of collision. Removing or reconstructing the historical mathematics erases a key element from the historical narrative of the development of the rules of collision.

Section 2
Isaac Beeckman's mathematical studies of the loss of motion from collision

2.1 – Beeckman's Question

> *Motion in a vacuum never increases, but decreases. Why, may I ask, then is universal rest not brought about*?[65]

Beeckman posed this question in a marginal heading at the end of the first mathematical study of collision that he included in his *Journal* (1618). This study had begun with the title: "In what manner motion in a vacuum is impeded by means of collision."[66] Its purpose was to mathematically investigate the amount of motion lost upon every collision. With enough collisions, universal rest would be brought about. But nature is not universally at rest. Things move. Thus Beeckman concluded: "so with this [the above mathematical account of collision] in place, motion in a vacuum can never be intelligible. Faster motions, can be drawn to an end."[67] Every entry in Beeckman's *Journal* that includes a mathematical investigation of collision takes on this problem of the consequential loss of motion. Among his *last* discussions of collision in the *Journal* (1634),[68] for example, Beeckman was still grappling with this mathematically justified puzzle at the heart of his philosophy of nature. In a marginal heading he wrote: "Can it be that motion in a vacuum could increase?"[69] Again, he cites mutual collisions causing rest. "Therefore," he asks, "why doesn't everything in the end rest? Can it be that fire has an

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[65] Beeckman, *Journal* 1:266-7. *Motus in vacuo nunquam crescit, sed decrescit. Cur igitur tandem non fit universalis quies?*
[66] Beeckman, *Journal* 1:265-6. *Motus in vacuo ab occurrentibus quomodo impediatur.*
[67] Beeckman, *Journal* 1:266. *His ita positis nunquam motus in vacuo potest intelligi ad celeriorem motum vergere*. Not only are fast motions brought to a standstill, but all bodies currently at rest are on account of the motions of equal collisions. See Beeckman, *Journal* 1:266. *Omnia tandem spectare ad quietem propter aequales occursus.*
[68] His final comments are found in two sections, one dated between August and 15 October 1634, and the other dated betweeen 15 October and November 1634.
[69] Beeckman, *Journal* 3:363. *Motus an in vacuo crescere possit.*

influence to some extent? Or can it be that the very action of reflection in a vacuum is to such a degree unknown?"[70]

These questions were particularly significant for Beeckman. His explanations of natural phenomena relied on the motion and interaction of corpuscles and ultimately of atoms. Beeckman was among the first in Europe to be committed to the program of explaining all natural changes in terms of corpuscular mechanisms, based on their size, shape, arrangement, and motion.[71] The term Beeckman used to describe this was "physico-mathematics." As Gaukroger and Schuster have shown, in 1618 (the same year as Beeckman's first mathematical investigations of collision), Beeckman met Descartes, and together they worked on problems in mathematics, mechanics, and music theory; and they applied physico-mathematical corpuscular explanations to the hydrostatics of Stevin.[72] The interaction of small bodies was a fruitful *explanans* of natural phenomena. However, the explanation of these interactions themselves posed a significant problem for Beeckman. His investigations into the collision of the smallest parts of matter seemed to have as a consequence that motion would be lost upon each impact. This was significant enough that Beeckman claimed to have:

> An argument against those who admit atoms, and so indeed against myself, this objection, which by my judgment is so great that I cannot solve it.[73]

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[70] Beeckman, *Journal* 3:364. *Cur non igitur tandem omnia quiescunt? An ignis hic aliquid potest? Aut an ejus actio tam est ignota quam reflectio in vacuo?*
[71] Stephen Gaukroger and John Schuster, "The hydrostatic paradox and the origins of Cartesian dynamics" *Studies in History and Philosophy of Science* 33 (2002): 551. Also see John D. North, "Stars and atoms," in *Thomas Harriot: An Elizabethan Man of Science*, ed. by Robert Rox (Burlington: Ashgate Publishing, 2000) 192. North claims that a source of what he calls Beeckman's "crude corpuscularianism" comes from the introduction to Hero of Alexandria's *Pneumatica*. As we will see below, Beeckman was likely familiar with this work, since it is on a list of texts that Snel had recommended to Beeckman. North also notes that Lucretius may well have been an influence on Beeckman and "[o]ne should remember that Lucretius was less pernicious than the other atomists in Christian eyes, since in Book V of *De rerum natura* he maintained that the world had a beginning and would probably have an end." See North, "Stars and atoms," 215.
[72] Gaukroger and Schuster, "Hydrostatick paradox," 550-8.
[73] Beeckman, *Journal* 2:100. *Argumentum contra eos qui atomos <admittunt>, atque adeò contra meipsum, objicio hoc, quod meo juditio tantum est ut ipse nequeam solvere.*

Connected to the loss of motion for Beeckman was the issue of reflection. Atoms were by definition the smallest parts of matter. As such they cannot be compressed. Since they are absolutely hard and cannot compress, Beeckman did not think they could spring off each other. Atoms would move together after meeting. But, as Beeckman admits, "reflection is indeed observed in an infinite multitude of phenomena, it seems that the doctrine of atoms is fundamentally overthrown by these phenomena."[74] Collision as an *explanans* was fruitful in Beeckman's physico-mathematical account of nature. Collision as the *explanandum* was problematic.

The collision of the smallest parts of matter seemed to entail that motion is lost in each collision and that no rebound is possible. This conflicts with the obvious observations that nature is full of motion and rebound. Beeckman's studies of collision are mathematical investigations of the amount of motion lost in each collision.

2.2 – Mathematical Education and Practical Mathematical Vocation

While Beeckman was in Leiden studying theology (1607-1610), he met with Rudolph Snel (1546-1613) who gave Beeckman a mathematical reading list. See table 4. In the passage of the *Journal* in which Beeckman explained the organization and content of the list, Beeckman claimed that anything he grasped on the subject of mathematics was

[74] Ibid., 2:100. Rather than rely on atoms themselves, Beeckman attempted to devise an elemental theory, with traditional elements similar to those of Aristotle, built from collections of atoms held together by various structures. Different arrangements of "congeries" of atoms determines the different elements. These congeries were the functional units of explanation, which allowed Beeckman to deemphasize atoms themselves as the unit of explanation. See Gaukroger and Schuster, "Hydrostatic paradox," 553-554. Also see John Schuster, "Descartes and the Scientific Revolution, 1618-1634: An Interpretation" (PhD diss., Princeton University, 1977) 62. Beeckman's mathematics of collision was on the level of *atoms*. He did, however, have some qualitative comments about the collision of congeries of atoms (corpuscles) as well. Since these congeries could have interstitial vacua, Beeckman speculated that they, unlike the atoms from which they are composed may rebound after meeting. Beeckman did not, however, explain the elastic bonds that would be required to hold these congeries together.

because it had been "soaked up" [*haurire*] from these books.[75] Snel had known Petrus Ramus while a student in Marburg, when Ramus had visited in 1596 and 1570.[76] Ramus had advocated severe pedagogical reform, targeting what he considered to be the uselessness and pointless complexity of Scholastic philosophy. Two years later Ramus was killed in Paris during the St. Bartholomew's Day Massacre.[77] The same year Snel began to lecture on the philosophy of Ramus at a Calvinist university in Marburg.[78] Later, while a physician in his hometown, students from Leiden asked Snel to give them lectures on mathematics. In 1580 he was granted a position at the university, "but on the condition that he would be dismissed as soon as another mathematician turned up who had more experience or a better reputation."[79] According to Ramus, mathematics should

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[75] Beeckman, *Journal* 4:19. "These were the authors that, having been asked by me at that time, father Snellius indicated to me "Mathesin" to be studied, with the prior he had ordered me to divide the mathematical art into its parts, as I have shown in the first column; which follow the very thing he wrote. Nothing beyond these did he produce anything of assistance to me, not because he might have refused, but because I had not asked. And therefore I was in front of necessary work, anything I grasped was soaked up from these books." *Hi fuerunt auctores quos Snellius pater olim a me rogatus, mihi indicavit ad Mathesin exercendam, cum prius jussisset me dividere artem methematicam in suas partes, quod feci uti videre est in prima columna; quæ sequuntur ipse scripsit. Neque præter ea mihi quicquam auxilij tulit, non quod denegaverit, sed quod ausus non essem rogare. Ideoque necessarium fuit pro labore, quicquid teneo, ex ijs libris haurire.*

[76] Theo Verbeek, "Notes on Ramism in the Netherlands," in *The Influence of Petrus Ramus: Studies in Sixteenth and Seventeenth Century Philosophy and Sciences*, ed. by Mordechai Feingold, Joseph S. Freedman, and Wolfgang Rother (Basel: Schwabe, 2001) 38-9.

[77] Foxe, John. *Acts and Monuments*, vol. 12 (London: John Day, 1583) 2153. "The bodies of the dead were caryed in Cartes to be throwne in the Riuer, so that not onely the Riuer was all steined therwith, but also whole streames in certayn places of the City did runne with goare bloud of the slayne bodyes. So greate was the outrage of that Heathenish persecution, that not onely the Protestantes, but also certayne whome they thought indifferent Papists they put to the sword in sted of Protestantes. In the number of them that were slayne of the more learned sort, was Petrus Ramus, also Lambinus an other notorious learned man, Plateanus, Lomenius, Chapesius, with others." The editors of John Foxe's The Acts and Monuments Online provide a biographical sketch of Pierre de la Ramée (Petrus Ramus) as well as a more detailed account of his death quoted from the *Dictionnaire général de biographie et d'histoire* (Paris, 1869). "Ramus died on the third day of the massacre (26 August), being sought out by a band of assassins encouraged (it was alleged) by a university rival, Pierre Charpentier, in his university rooms in the Collège de Presles, from where he was thrown out of the window, still alive, and dragged by his feet to be dumped in the river Seine."

[78] Klaas van Berkel, "A note on Rudolf Snellius and the early history of mathematics in Leiden," in *Mathematics from Manuscript to Print* 1300-1600, ed. by Cynthia Hay (Oxford: Clarendon Press, 1988) 157. According to Berkel, Snel's students were so impressed that, without his knowledge or consent, they published his lectures on Ramus.

[79] Berkel, "A note on Snellius," 157.

be little more than a systematic treatment of the methods used by merchants, navigators, surveyors, and engineers. Snel would likely have been sympathetic to such reforms, but he also thought that it was possible to gain *new knowledge* that was not already contained in the classical texts and corpus of mathematics by looking at the work of merchants, craftsmen, and musicians.[80]

[80] Ibid., 157. "Ramism as taught by Snellius was a plea for breaking down the social and intellectual barriers between the theoretical science inside the university and the practical arts outside."

Mathematical authors recommended to me by father Snellius	SIMPLE AND MIXED MATHEMATICS			
	Simple	Geometria		Ramus[81]
				Euclid
				Hero
		Arithmetica		Ramus[82]
				Boethius
				Euclid
	Mixed	1.	Astronomia	Ptolomy
				Copernicus
			Astrology	Ptolomy[83]
				Hermes
			Gnomonica	Ptolomy <de> *Analemmate,* Comandinus
				Clavius
				Johan Baptista
			Meteoroscopia	Regiomontanus
			Dioptrica	Hero
		2.	Optica Catoptrica	Euclid
				Ptolomy
				Vitello
			Sciagraphia	Stevin
				Comandinus
		3.	Geodaesia	Hero
			Cosmographia	Orontius
				Ptolomy
			Chorographia	sub Geographia
		4.	Canonica, id est Musica practica	Glareanus
		5.	Arithmetica practica	Ramus
				Calvinius
				alij.
		6.	Mechanica	Hero
				Comandinus
				Pappus

Table 4. This table is a transcription and translation of Beeckman, *Journal* 4: 17-19 (282 recto). The editor of the *Journal* has researched the specific texts that would have been available to Beeckman, some of which Beeckman explicitly cited in his *Journal*. I have only noted three.

Snel's taxonomy, in the list he gave to Beeckman, expands on the ancient framework attributed to Geminus by Proclus and reiterated by Clavius, mentioned in section 1. It also provides a more practical orientation to the areas of "mixed

[81] *Praelectiones in Geometriam Rami* published by Snellius (Francof., 1590).
[82] *Rami Arihmeticae libri duo cum explicationibus* published by Snellius (Francof., 1596; ibid. 1599).
[83] Beeckman also used Cardano's commentaries on Ptolemy. See Beeckman, *Journal* 2:136, particularly the entry for 11 November 1620.

mathematics." Rather than use the *intelligible/sensible* distinction, Snel used "simple" and "mixed." Geometry and arithmetic are the two pillars of mathematics, but they are not referred to as "abstract" or "pure." And they are represented primarily by Snel's own works on Ramist arithmetic and geometry.[84] The mixed mathematics are organized into six groups, which correspond in outline to the six groups of Geminus' *sensibles*—the (1) mathematics of the heavens (*astronomia*), (2) the mathematics of light (*optica*), (3) the mathematics of the land/mapping (*geodesia*), (4) practical music (*musica*), (5) practical arithmetic (*logistica*), and (6) mechanics (*mechanica*). Although "mixed mathematics" does not necessarily imply any practical application (only that the principles of the mathematics are mixed with principles drawn from nature), in Snel's categorization we see a distinct "practical" focus, particularly in the subfields of the first three groups. For instance, the first group, which I have called the mathematics of the heavens, contains not only astronomy, but the study of astrology which served as an important tool for medicine as well as the prediction of the weather. Also included in this group is the study of instruments – an entire branch dedicated to the sundial (*gnomon*) and another dedicated to various other astronomical instruments used to find the meridian, the elevation of the pole, the obliquity of the equator, and the position of the stars (*dioptrica*), which would have been useful in navigation and cartography.

Beeckman had intended to become a minister.[85] After his theological studies at Leiden, Beeckman stayed for several months at the Huguenot academy of Saumur (1612)

[84] *Rami Arithmeticae libri duo, cum explicationibus* (Francof., 1596; ibid. 1599); *Praelectiones in Geometriam Rami* (Francof., 1590).

[85] Beeckman had been in Leiden to study theology. He was there when a set of famous debates were taking place between two members of the theology faculty, Jacobus Arminius (1560-1609) and Franciscus Gomarus (1563-1641), which would create a rift in the Dutch church between those who would become known as the Arminians or Remonstrants, and the Gomarists or the Contra-Remonstrants. Arminius argued for reforming some Calvinist tenets, particularly the doctrine of predestination. Upon his death his

and was accepted as "proponent of the ministry" in 1613.[86] However, Beeckman could not find a place as a minister.[87] Instead he entered the profession of his father, becoming a candle-maker. He also repaired the water-works in breweries and gardens.[88] In 1618 he received a medical degree from Caen in France. The dissertation was on fevers, but the speech he gave prior to his dissertation defense was primarily about mathematics, its relationship to nature and other fields of study, and the limits of reason.[89] Soon after he held a position as conrector at the Latin school in Veere, where he also laid water conduits and worked on the dikes. This was followed by a position as conrector in Utrecht. At this time he had several exchanges with burgomasters and magistrates, who were craftsmen themselves, providing them advice on technical improvements in navigation, advice on drainage, and advice on the building of mills. In 1620 he acquired another position as conrector, this time in Rotterdam. Here, in conjunction with some craftsmen, the physician Fornerius, and the mathematician Stampioen, he founded a

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followers issued five articles of remonstrance (protest). In 1618 the Dutch Reformed Church met, led by Leiden professor Gomarus, at the national Synod of Dort to settle the issue, ultimately rejecting each of the five articles of remonstrance as heresy. Beeckman strongly maintained a Calvinist contra-remonstrant doctrine, even opposing the wedding of his sister to his own friend, Justinus van Assche, who supported the views of Arminius. See R. Hookyaas, "Science and Religion in the Seventeenth Century: Isaac Beeckman (1588-1637)," *Free University Quarterly* 1 (1950): 175.

[86] Hookyaas, "Science and Religion," 170.

[87] Beeckman did not find a place as a minister in his hometown of Middelburg or anywhere in the province of Zeeland. This was due to the reputation of the Beeckman family name. His father strongly opposed the practice of baptizing the children of "Romish" parents, which the leading theologians and ministers in Zeeland advocated. See Hookyaas, "Science and Religion," 171. Also see Klaas van Berkel, Albert van Helden, and Lodewijk Palm, eds., *A History of Science in the Netherlands: Survey, Themes and Reference* (Boston: Brill, 1999).

[88] Gaukroger and Schuster, "Hydrostatic paradox," 553n.

[89] Beeckman, *Journal* 4: 40-45. In the speech Beeckman divides philosophy into a mathematical part and physical part. Although he admits that physical knowledge is more prestigious, he maintained that to obtain any physical knowledge, the need for mathematics is very great. Prior to giving the speech at the University of Caen, Beeckman seems to have first rehearsed several of the ideas, which can be found in Beeckman, *Journal* 1: 131-2.

Collegium Mechanicum. In 1627 he accepted an offer to become rector of the School of Dordrecht. Beeckman's inaugural lecture was on his *philosophia physico-mathematica.*[90]

2.3 – The Proportions of Machines

Just prior to Beeckman's first mathematical study of collision in his *Journal*, and just prior to the defense of his dissertation at the University of Caen, Beeckman wrote on the relationship between faith and reason, and theology and philosophy. He divided philosophy into two parts: "one considers the essence of things" and "the other considers the proportion things have to each other." The former he called "physics" and "the latter mathematics and mechanics."[91] The proportion can be between numbers, or weights, or lengths, or speeds, or anything that is a quantity. According to Beeckman, mathematics and mechanics, unlike physics, is about the relationship between quantities.

In Beeckman's studies of collision, he relied on proportions, wherein the relations were between elements of machines. The material constraints of the machine itself had been removed, but the relevant proportions remain.[92] For instance, to establish the

[90] Beeckman, *Journal* 4: 122-126. Beechman gave an inaugural speech when he was made rector of the School of Dordrecht (recorded in his *Journal* in June 1627). Also see Berkel et al., *History of Science in the Netherlands,* 411. Hookyaas, "Science and Religion," 172.

[91] Beeckman, *Journal* 1:132.

[92] Bertoloni Meli has referred a broader pattern of transformation in seventeenth century mechanics, called "dematerialization," in which Beeckman's work is an example. "Dematerialization" was particularly important to the study of collision. It refers to "transformations involving the removal of material constraints through a process of mental abstraction ... the same proportions or relations valid for the constrained case were supposed to remain valid also in the unconstrained one." Bertoloni Meli describes two other patterns of transformation. These include "unmasking" and "morphing." The first involves "the recognition that apparently complex and elaborate objects or devices can be shown to consist of simple, known ones in disguise, as in a metaphorical removal of a veil or a mask. In these cases simple visual inspection—at times with minimalist interventions—enabled the reduction of several seemingly intractable cases to established ones. The term "unmasking" captures the minimal intervention required in these cases." "Morphing" on the otherhand, "required some degree of intervention and elaboration: the issue was not simply to point to a different way of looking at an object by metaphorically removing a veil or a mask, but to perform a series of operations—in line with my characterization of thinking with objects, either mentally or experimentally, with thought and real experiemtns—leading from one object or device to another."

principle that the largest weight is moved in a vacuum by means of the smallest force, he used the example of the difficulty of moving various weights hung by different lengths of cord. First, Beeckman noted that a weight hung by a cord is moved easily by a light impulse [*momentum*]. However, using the same cord, but doubling the weight, will require a "not so light impulse." But, if the doubled weight is hung by a cord of doubled length, then it will be moved equally by as light of an impulse as the former. The cause of the difficulty of moving a body is the pull of the center of the earth. In a vacuum, Beeckman says, nothing is pulled back by means of the center of the earth. This, presumably, is suggested by imagining a weight hung by longer and longer lengths of rope.[93]

Beeckman claimed that the same holds true (that the largest weight will be moved by means of the smallest force, in a vacuum), if a resting body should be touched by a moving body. However, Beeckman claimed that the body that was resting will be moved with the following stipulation [pacto]: "If each is of equal body [*corporeitatis*], both will be moved twice as slow as the former was moved."[94] This pacto (and its explanation), will be of central importance to his broader account of collision, or, as he puts it in a marginal note: "In what way motion in a vacuum is impeded by means of collision [*occurrentibus*]."[95] He provided a line of reasoning for this. The two bodies have the same quantity. Since the two bodies move together after meeting, the same impetus [*impetus*] that moved the first body, now must sustain twice as large a body as before.

"...the term 'morphing'...capture[s] these creative transformations." See Domenico Bertoloni Meli, "Patterns of Transformation in Seventeenth-Century Mechanics." *The Monist* 93 (2010): 579.

[93] Beeckman, *Journal* 1:265. December 1618. The following is the marginal note that introduces the first instance of Beeckman's work on collision: *Pondus maximum in vacuo a minima vi moveri probatur.*

[94] Beeckman, *Journal* 1:265-6. *Quod quiescebat movebitur cum moto hoc pacto: Si utrumque est aequalis corporeitatis, utrumque movebitur duplo tardiùs quàm priùs motum movebatur.*

[95] Beeckman, *Journal* 1:265-6. *Motus in vacuo ab occurrentibus quomodo impediatur.*

Necessarily, it will proceed slower proportionally.[96] This line of reasoning is true, according to Beeckman, because of the proportions that one observes in machines:

> For it is observed [*animadvertitur*] in all machines, as *twice the weight, having undergone an equal force, the same ascends twice as slow as the first weight.*[97]

The proportions are rooted in machines. Not only is the general claim—"the largest weight is moved in a vacuum by means of the smallest force"—justified by thinking about machines, but his specific *pacto* is also fundamentally a proportion rooted in machines.

The quantities used in the above statement do not appear to map obviously onto the collision of bodies. In the former he describes weight, force, and a more slowly *ascending* motion. In the latter he describes "body" [*corporeitas*][98] – not weight [*pondus*] – and motion with no mention of ascending. The latter could be described as a "dematerialized balance." The proportions remain valid, but the material constraints have been removed. Moreover, it is a very specific treatment of the law of the lever as found in the pseudo-Aristotelian[99] work, *The Mechanical Problems*. [100]

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[96] Beeckman, *Journal* 1:266. "Truly with so many parts belonging to the resting body as the moving body, and it brings an equal progressing motion to them, it is with the same impetus [impetus] that it must sustain twice as large a body as before, necessarly it is to proceed slower by so much..." *Cùm enim tot partes insunt quiescenti ac moto, et motum aequalem progressum illi adfert, id est cùm idem impetus debet sustinere duplò majus corpus quàm antè, necessè est tantò etiam tardiùs procedere...*
[97] Beeckman, *Journal* 1:266. Emphasis in original. *[I]d enim in omnibus machinis animadvertitur, ut* duplex ponduss aequali vi sublatum, etiam duplò tardiùs ascendat quàm prius pondus.
[98] Note that this word is not *corpus, corporis*. Rather, it is *corporeitas, corporeitatis*. Beeckman appears to be referring to the amount of material in a body. The *corporeitas* does not necessarily correspond to the amount of space the body occupies, and he claims that it is possible for bodies occupying a smaller space to have a larger *corporeitas* than bodies occupying a larger space. See Beeckman, *Journal* 2:52.
[99] Thomas Winter has argued that the *Mechanical Problems* was authored by Archytas. See Thomas Nelson Winter, "The Mechanical Problems in the Corpus of Aristotle," *Faculty Publications, Classics and Religious Studies Department* Paper 68 (Lincoln: University of Nebraska, 2007), accessed January 21, 2015, http://digitalcommons.unl.edu/classicsfacpub/68.
[100] Gaukroger and Schuster, "Hydrostatic paradox," 555; Schuster, "Descartes and the Scientific Revolution (dissertation)," 66-7. For a discussion of the importance of the lever, as well as inclined planes, pendulums, springs, and strings, in mechanics and natural philosophy, see Domenico Bertoloni Meli, *Thinking with Objects* (Baltimore: The Johns Hopkins University Press, 2006).

In the opening pages of the pseudo-Aristotelian *Mechanical Problems*, the author directly connects mechanical movement to the lever, the lever to the balance, the balance to the circle, and the circle to the "marvelous" unity of opposites.[101] According to the author, the marvel of mechanical advantage stems from something even more marvelous—the unity of opposites in the circle.[102] The opposites to which the author refers include the following: the circle "derives from the moving and the standing, whose nature is opposite each the other;" "the perimeter...generates opposites: the hollow and the curved;" and "it moves backwards and forwards at the same time."[103] According to the *Mechanical Problems*, "many of the marvels about the motion of circles derive from the fact that, on any one line drawn from the center, no two points are swept at the same pace as another but always the point further from the motionless end is quicker."[104]

Problem 3 of *The Mechanical Problems* asks "Why is it that small forces can move great weights by means of a lever[?]"[105] The weight which is moved, to the weight which moves it, is inversely proportional to the lengths of the arms of the lever. The reason for this, according to the author of the *Mechanical Problems* is due to speed. Consider two circles, a larger and smaller circle with the same center. Imagine that both are drawn on the same disk. Now imagine a straight line drawn from the smaller circle, through the center and to the larger circle; at the intersections of this line and the circles

[101] [pseudo] Aristotle, *Mechanical Problems*, in *Minor Works*, trans. by Walter Stanley Hett (Loeb Classical Library, Cambridge, Harvard University Press, 1936), 334. "Everything about the balance is resolved in the circle; everything about the lever is resolved in the balance, and practically everything about mechanical movement is resolved in the lever." Translation by Winter, "Mechanical Problems," 2.
[102] Aristotle, *Mechanical Problems*, 332. "The circle contains the first principle of all such matters. This falls out quite logically: it is nothing absurd for a marvel to stem from something more marvelous still, and most remarkable is for there to be opposites inherent in each other, and the circle is made of opposites." Translation by Winter, "Mechanical Problems," 2.
[103] Aristotle, *Mechanical Problems*, 332. Translation by Winter, "Mechanical Problems," 2.
[104] Aristotle, *Mechanical Problems*, 334. Translation by Winter, "Mechanical Problems," 2.
[105] [pseudo] Aristotle, *Mechanical Problems*, in *Minor Works*, trans. by Walter Stanley Hett (Loeb Classical Library, Cambridge, Harvard University Press, 1936), 353.

are points. If one rotates the disk, the speed of the point on the larger circle will move faster than the speed of the point on the smaller circle. Now imagine that the center of the circle is a fulcrum and the radius of the smaller circle is the smaller arm of a balance, and the radius of the larger circle is the larger arm of a balance. The author of *The Mechanical Problems* claims that the speed of a body is inversely proportional to the weight. If the balance rotated, the longer arm of a balance would sweep out a larger arc of a circle than the short arm of a balance. Given the same force, a body at the end of the longer arm will move faster than that at the shorter arm. The equilibrium of the balance is understood similarly. The weights are inversely proportional to the speeds of the weights. A heavier weight closer to the fulcrum would move slower than the lighter weight farther from the fulcrum, if the balance rotated about the fulcrum in a circle. If these weights and speeds are inversely proportional, the balance is in equilibrium.

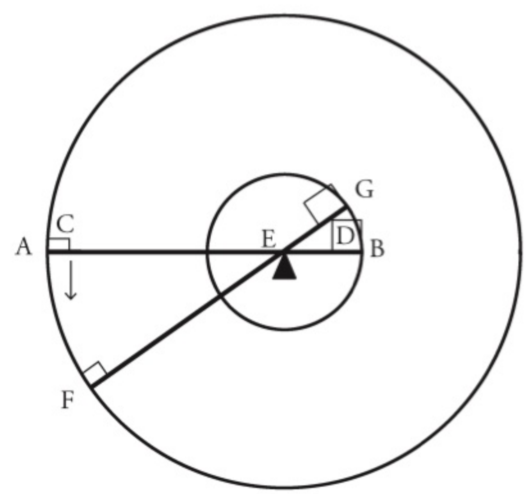

Figure 9. Image reproduced from Winter, "Mechanical Problems," 11.

If there are two weights, one twice as large as the other, the larger will ascend twice as slowly as the first weight, given the same force, since the radius of the circle that the larger weight sweeps out is twice as small as the radius of the circle of the smaller weight. Thus we can see the relationship between the concepts from what Beeckman observes in all machines and the collision of bodies. The "equal force," "weights" and "ascending slowly" are all from the *Mechanical Problems* tradition of explaining the action of the lever. In particular, the "Aristotelian" explanation of the lever provides a framework in which to understand the proportions between speed and weight of bodies.

In addition to the similarity of the concepts used, there are other reasons to think that Beeckman was working with ideas from this tradition of the *Mechanical Problems*, such as pedagogical traditions, and direct references to the text and its commentaries in the *Journal*. As noted in section 2.2, there is a pedagogical connection between Petrus Ramus and Isaac Beeckman, primarily through Snel. According to Paul Lawrence Rose and Stillman Drake, Ramus had lectured on the *Mechanical Problems* and "even overcame in this matter his aversion to Aristotle, whom he actually praised [in his *Scholarum Mathematicarum*] for having writing a mathematical work such as the *Mechanica.*"[106] Moreover, Beeckman had clearly read and studied the *Mechanical Problems* as we see in his *Journal* entry from 18 June 1619.[107] And after Bernardino Baldi's translation and commentary, *In mechanica Aristotelis problemata exercitationes*, was published 1621, Beeckman took notes on Baldi's studies of several of the

[106] Paul Lawrence Rose and Stillman Drake, "The Pseudo-Aristotelian *Questions of Mechanics* in Renaissance Culture," *Studies in the Renaissance* 18 (1971): 99-100. Petrus Ramus, *Scholarum Mathematicarum Libri XXXI* (Basel, 1569) 21.

[107] Beeckman, *Journal* 1:318. Beeckman's marginal heading is: "[A thing] is cut better by the motion of an ax than by the weight. Why?" *Securis motu meliùs secatur quàm pondere. Cur.* In the text Beeckman refers directly to problem 19. See Aristotle, *Mechanical Problems*, 375 for the corresponding passage. The editor of the *Journal*, Cornelis de Waard, includes the following note on the editions available to Beeckman: "Le texte grec des *Quaestiones mechanicae* fut publié déjà au vol. IV de l'édition des Oeuvres d'Aristote, publiée par Aldus Manutius (*Venise*, 1498), puis à la fin du vol. I des ΑΡΙΣΤΟΤΕΛΟΥΣ ἅπαντα, publiés par Erasme (*Basileae*, 1531; réimpr. 1539). On le trouve également au vol. VI de ΑΡΙΣΤΟΤΕΛΟΥΣ τὰ εὑρισκόμενα, publiés par Frid. Sylburgius (Francfort, 1585). On a aussi deux éditions a part: celle de *Paris, Wechel,* 1566, in-4^{o} et celle *Graece, emendata, Latina facta et commentariis illustrata ab* Henrico Monantholio (*Paris*, 1599), in-4^{o}."

questions.[108] In the commentary, Baldi criticized the dynamic arguments regarding the law of the lever. "[He] rejected the idea that the speed could be considered the cause of the working of the lever, because it was inconceivable to explain the equilibrium of the balance by referring to motion."[109] Beeckman appears to defend the *Mechanical Problems* tradition directly against this kind of criticism. In 1629 he wrote:

> The cause of equilibrium therefore can be motion, even if the bodies in equilibrium are not moved. For the cause of equilibrium is past and future motion. During the present, to be sure, the body is at rest because past and future motions occasion rest.[110]

There is also evidence that Beeckman agreed with Mersenne's explicit defense of the "Aristotelian" explanation of the balance.[111] Beeckman did so by referring to what he had written "a little before concerning motion."[112] He explicitly states: "Namely I said that the amount of matter [*corporeitatem*] and motion are reciprocal to each other. Thus the same must be concluded concerning the balance." *Sic etiam ratiocinandum de bilance.*[113]

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[108] Beeckman, *Journal* 2:278-81. Questions 2, 8, 17, 18, 24, and 32 are mentioned by name in his notes around December 1626 and March 1627.

[109] Elio Nenci, *Bernardino Baldi's In mechanica Aristotelis problemata exercitationes*, communicated by Jürgen Renn and Antonio Becchi, trans. by Adriano Carugo. Max Planck Research Library for the History and Development of Knoweldge Sources 3 (Edition Open Access, 2011): 19. Also see Paolo Palmieri, "Breaking the circle: the emergence of Archimedean mechanics in the late Renaissance" *Archive for History of Exact Sciences* 62 (2008): 306-7, 325-9. Palmieri argues that "Bernardino Baldi debunked the miraculous nature the circle, trying, as he saw it, to correct the errors of the pseudo-Aristotle by means of Archimedes and Guido Ubaldo."

[110] Beeckman, *Journal* 3:134. Translation quoted in Schuster, "Descartes and the Scientific Revolution (dissertation)," 68.

[111] The editor of the *Journal*, Cornelis de Waard, included the following note on the source of Mersenne's defense: *Livre second de l'Harmonie univerrselle. Où l'harmonie de toutes les parties du Monde est expliquee tant en general qu'en particulier. Par le Sieur DE SERMES (marque d'imprimeur). A Paris, pour Guillaume Baudry, Rue des Amandiers. près le College des Grassins. M. DC. XXVII. Avec privilege du Roy.* – in-8^{o}; 18pp. suivies des pp. 305-477.

[112] Beeckman, *Journal* 3:128-9. He seems to be referring to what he had written earlier that month on 13 September 1629. Beeckman continues: "For it follows from them [what he had written a little before] that a sphere twice as heavy [as another sphere], that is, having twice as much matter, but moving twice as slowly [as the other sphere], will be stopped after colliding with it, that is, both spheres will be at rest" (Beeckman, *Journal* 3:133), translation quoted in Schuster, "Descartes and the Scientific Revolution (dissertation)," 66.

[113] Beeckman, *Journal* 3:133. *Dictum est enim corporeitatem et motum inter se reciprocari. Sic etiam ratiocinandum de bilance.*

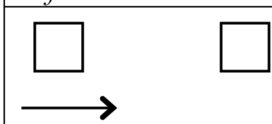
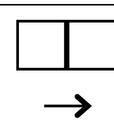
2.4 – Collision and the pseudo-Aristotelian balance

The reciprocal relationship between motion and *corporeitatem* on the model of the balance as understood by the pseudo-Aristotelian *Mechanical Problems* is the key to the mathematics of Beeckman's studies of collision of atoms. The bodies (*corporeitatem*) are inversely proportional to the speeds of the bodies.

$$c_1 : c_2 :: s_2 : s_1$$

c_1 is the size of the body initially in motion and c_2 is the aggregate size of the two bodies involved. s_1 is the speed of the body initially in motion and s_2 is the speed of the two bodies moving together. Recall that the *aggregate size of the two bodies* is the relevant quantity for Beeckman because the smallest parts of nature cannot compress. Thus, the bodies do not rebound. The two bodies move together after the meeting. So, under this relation from the pseudo-Aristotelian balance, given equally sized bodies, one initially in motion and one initially at rest, the motion of the aggregate will be twice as slow as the initially moving body. This inverse proportion from the pseudo-Aristotelian balance maps onto Beeckman's *pacto* for the collision of bodies.

> And the same now if the resting body should be touched by a body having been set in motion whithersoever. Now the body that was resting will be moved by the moving body with this stipulation [pacto]: *if each is of equal body, both will be moved twice as slow as the former was moved.*[114]

pacto	
before bodies meet	*after bodies meet*
 *s*	 *s*/2

[114] Beeckman, *Journal* 1:265-6. 23 November - 26 December 1618. *Idemque si quod corpus quiescens a quocumque corpore moto tangatur. Quod quiescebat movebitur cum moto hoc pacto:* Si utrumque est aequalis corporeitatis, utrumque movebitur duplo tardiùs quàm priùs motum movebatur.

In the subsequent paragraphs in Beeckman's note from December 1618, all organized the marginal title "In what way motion in a vacuum is impeded by means of collision," Beeckman describes various combinations of sizes and speeds of bodies. The intention is *not* to systematically present "rules of collision" as some historians have claimed,[115] rather it is to show precisely that motion is impeded in every case.

In those collisions in which one body is initially at rest, he considers instances where the *larger* body is at rest and instances where the *larger* body is initially in motion. In the framework of the former, he determines the impeded speed resulting from a collision where the larger resting body is twice the size, and another where the larger resting body is three times the size. In keeping with his motivation to show in what way motion in a vacuum is *impeded* by means of collision, the focus of these calculations is on how much is *removed* [*demuntur*] from the initial speed. In the case where the larger resting body is twice the size, 2/3 is removed from the initial speed, in the latter case where the larger body is three times the size, ¾ is removed. The focus on the amount *removed* appears very deliberate. In fact, it requires an additional (although quite simple) step in the calculation. Since there is a reciprocal relationship between body and speed, if the body after collision is thrice the size, then the speed will be a third the original. Rather than state that the motion after collision is a third the speed of the motion before collision, Beeckman focuses on how much must have been removed to produce such a speed.

[115] For instance, see Gaukroger and Schuster, "Hydrostatic paradox," 555-6.

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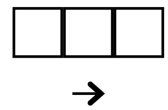
s</td><td>
$s/3$</td></tr>
<tr><td colspan="2">Larger body is initially at rest. Larger body is twice the size of the smaller.
The motion of the aggregate, c_2, is 1/3 the speed of the body initially moving.
2/3 is removed from the initial speed because of the collision.</td></tr>
</table>

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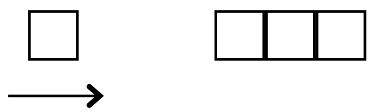
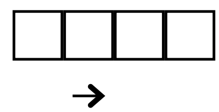
<tr><td>before bodies meet</td><td>after bodies meet</td></tr>
<tr><td>
s</td><td>
$s/4$</td></tr>
<tr><td colspan="2">Larger body is initially at rest. Larger body is three times the size of smaller.
The motion of the aggregate, c_2, is 1/4 the speed of the body initially moving.
3/4 is removed from the initial speed because of the collision.</td></tr>
</table>

In the framework where the larger body is initially moving, he considers the situation where the larger initially moving body is twice the size. Again the focus is on how much is removed, in this case 1/3 of the motion, even though the final speed (2/3 the original) is more directly attainable from the proportion.

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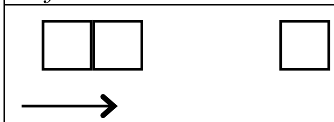
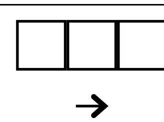
<tr><td>
s</td><td>
$\frac{2}{3}s$</td></tr>
<tr><td colspan="2">Larger body is initially moving.
Larger body is twice the size of the smaller body (at rest).
Motion of the aggregate, c_2, is 2/3 the speed of the body initially moving.[116]
1/3 is removed from the initial speed because of the collision.</td></tr>
</table>

[116] $c_1 : c_2 :: s_2 : s_1$. Let $c_1 = 2m$. Let $c_2 = 3m$. Since $c_1 s_1 \propto c_2 s_2$, and since $s_2 = \frac{2}{3}s_1$, then $2ms_1 = 3m\frac{2}{3}s_1$.

Beeckman then presents scenarios in which both bodies are initially in motion. In 1618 he wrote, without any additional explanation, that when "equal bodies meet *mutually* with an equal speed to each other, they will exactly rest, each with an abrogated motion."[117] When he returned to the subject in 1620, he provided more explanation: "the one will remove [*auferet*] the motion of the other; indeed neither will propel the other, since neither remains with any [motion] in the circumstance by means of the other."[118] In 1620 he explicitly specified that he is referring to the smallest parts of nature: bodies without pores, or those that in general cannot be "reflected" [*non possunt reflecti*]. In other words his notes are about perfectly hard atoms with no interstitial void. Interestingly, in this context he claimed that if he were discussing bodies with pores or "connected bodies" *i.e.* his *micro-corpuscles*, then there would be need to include an account of reflection. Regarding "connected bodies," he states, in passing, that they would be reflected to such a degree as each was impinging the other, with nearly the same speed and from the same way that they had come.[119] It is important to note that they would be reflected with only *nearly* [*fere*] the same speed. He did not discuss the collision of these "connected bodies" at length, nor did he provide any mathematical examples of reflecting bodies. They are only mentioned in passing.

The notion that the motion of a body "removes" or "carries off" the motion of another body is relied upon in his final scenarios, where both bodies are in motion. He used this idea to transform the situation where equal bodies have different speeds into one

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[117] Beeckman, *Journal* 1:266. *Quae aequalia aequali celeritate sibi* mutuo *occurrunt, directe quiescent, abrogato utriusque motu*.

[118] Beeckman, *Journal* 2:45. *At si duo corpora aequalia aequali motu in vacuo secundùm rectam lineam sibi invicem occurrant, in ipso occursu quiescent et alterum alterius motum auferet; neutrum enim alterum pellet, cùm neutrum aliquâ in re ab altero superetur.*

[119] Beeckman, *Journal* 2:45. *[Q]uod si fiat, poterunt tam apti esse ad reflectionem ut utrumque alteri impingens, fere eadem celeritate et eadem via, qua venerat, reflexum redeat.*

of the kinds already discussed, namely where one body is in motion and one is at rest. So for example, he wrote that given equal bodies with unequal speeds, "if they meet each other mutually, the smaller speed is removed from the larger and the excess moves with half the subsequent motion as the fast part was moved."[120] When Beeckman returned to the topic in 1620 he provides a detailed example of this scenario.

> If therefore equal bodies mutually meet with each other in a straight line with unequal speeds, the first smaller speed is removed from the larger and the body is then considered as resting and would be brought together with the remaining speed of the larger speed, from which the smaller speed was removed through subtraction. For example: If one body is moved in the same time through four stadia, where the other [is moved] through two stadia, in the collision of these bodies they must remove two stadia from the four, and the two which remain, will be divided in two; these conjoined bodies are moved therefore in the same time by a single stadium. However you will understand that the smaller speed must be removed from the larger [speed], if you will consider that in equal bodies an equal motion itself is mutually removed; therefore this part of the speed resists it by an equal speed, with the remaining speed not impeded.[121]

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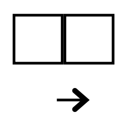
v_1 v_2</td><td>
$\frac{v_! - v_2}{2}$</td></tr>
<tr><td colspan="2">Bodies same size. Both initially in motion, but different speeds.
Motion of the aggregate, c_2, is the difference of initial speeds, divided by 2.</td></tr>
</table>

The outcomes described thus far in Beeckman's account of collision are consistent with those produced by modern equations of the conservation of momentum for inelastic equations.

[120] Beeckman, *Journal* 1:266. *At si sibi mutuò occurrant, aufertur minor celeritas à majore moveturque utrumque secundum dimidium motum excessûs versus quam partem celeriùs movebatur.*

[121] Beeckman, *Journal* 2:46. *Si igitur aequalia corpora sibi invicem occurrant in lineâ rectâ inaequali celeritate, primum aufertur minor celeritas à majore et id corpus tum consideratur ut quiescens conferturque cum residuâ celeritate majoris celeritatis, unde minor celeritas per sub | stractionem ablata fuit. Exempli gratiâ: Si unum corpus eo tempore moveatur per quatuor stadia, quo alterum per duo stadia, in concursu horum corporum auferenda sunt duo stadia à quatuor, et duo quae restant, bisecanda; movebuntur ergo corpora haec conjuncta eodem tempore per unicum stadium. Auferendam autem esse minorem celeritatem à majore intelliges, si consideraveris in aequalibus corporibus aequalis motûs se mutuò auferre; hîc igitur ea pars celeritatis resistit aequali celeritati, residuâ celeritate non impeditâ.*

$$m_1 v_{1i} + m_2 v_{2i} = (m_1 + m_2) v_f$$

(ia) Bodies the same size. One initially in motion, the other initially at rest
(ib) Larger body is initially at rest. Larger body is twice the size of the smaller
(ic) Larger body is initially at rest. Larger body is three times the size of the smaller
(ii) Smaller body is initially at rest. Larger body is twice the size of smaller body
(iii) Both bodies initially moving with same speed, and the same size
(iva) Bodies the same size. Both bodies initially moving with different speeds

For example, even the two-step approach taken in the (iva), where Beeckman first "transforms" the scenario into (ia), produces the same numerical result as would be found using the conservation of momentum in inelastic collisions.

$$m_1 = k = m_2$$
$$let\ |v_{1i}| = 4$$
$$let\ |v_{2i}| = 2$$
$$kv_{1i} - kv_{2i} = (k + k)v_f$$
$$v_f = \frac{4-2}{2}$$

Beeckman provided two more examples in his notes that are not consistent with the strategy he took in the above six scenarios. They also do not happen to be consistent with the modern equation for the conservation of momentum for inelastic collision.

(ivb) Larger is twice the size, but the bodies move with equal speed
(ivc) Larger is twice the size, and the smaller body is slower by twice

The strategies that Beeckman employed in the previous six scenarios are not applicable. The original framework, derived from the balance, requires that one body be at rest. He extended this framework to include two equal bodies moving with various speeds by having one motion "remove" the motion of the other. For instance, (iva) is transformed into (ia). In these latter two scenarios both bodies are in motion and the bodies are not the same size. Regarding (ivb) Beeckman wrote:

> The larger body by twice, if it meets the other [body] equally in swiftness, destroys[122] half the speed and indeed carries it off by means of itself; the remaining speed is divided in two and each is moved slower by four than the larger body was moved before.[123]

According to the modern equation, each body should be moved (together) slower by *three* than the larger body was moved before.[124] Beeckman, on the other hand, concluded that each body would be moved slower by *four* than the larger body was moved before. A plausible explanation of Beeckman's rationale is that he appears to note that the ratio of body to speed in the smaller moving body is half that of the ratio of body to speed in the larger moving body. Thus ½ the speed is "destroyed" when they meet. This ½ speed is then divided between the two bodies involved, to produce a speed that is ¼ the original. If this is the rationale he used, it is not entirely clear to me why he neglected the sizes of the bodies at this step.[125]

Beeckman revisited the topic of collision (*viz.* motion impeded by means of collision) multiple times throughout his *Journal*. In 1620, for example, he drew many of the same conclusions. In addition, he included new ideas regarding limiting cases, he also addressed *in general terms* a scenario akin to (ivb). Notably, he concluded *different* results.

> However, if the bodies are unequal, in truth equal speeds, the smaller body is removed from the larger and then the smaller is considered as resting. If therefore

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[122] Note the word being used here – *perdit* (from *perdo, perdere*) meaning "to destroy" and *rapit* (from *rapio, rapere*) meaning "to seize, to carry off." Clearly this is in keping with Beeckman's investigation of the manner in which motion is hindered or impeded.

[123] Beeckman, *Journal* 1:266. *Duplò verò majus corpus, si alteri aequed) celeri occurrat, perdit dimidium celeritatis, et siquidem id secum rapit; reliquum dimidium bisecatur moveturque utrumque quadruplò1) tardiùs quàm majus corpus antè movebatur.*

[124] Given that $m_1 = 2m_2$, and $|v_{1i}| = s = |v_{2i}|$, then by the conservation of momentum for inelastic collisions: $2m_2s - m_2s = (2m_2 + m_2)v_f$. It follows that $v_f = \frac{m_2 s}{3m_2} = \frac{1}{3}s$.

[125] In the case of (ivb), one motion cannot "carry off" the motion of the other, to transform the scenario into another in which one body is at rest as he did for (iva). Since the speeds are equal, there would no motions, if one carried off the other.

> the remaining of the larger body is equal to the smaller body, each will be moved by half of the former speed, just as we before heard.[126]

This is substantially different from the above solution. If the larger body is twice the size of the smaller, then according to the entry from 1620, 1 is removed from 2 to produce 1. Therefore the remainder of the larger is equal to the smaller. Thus the smaller body (of size 1) is taken to be at rest. The larger body, now reduced to size 1, meets a body at rest which is also size 1. Just as was discussed in the first example (i), the speed of the initially moving body is divided by two. Obviously this is not equal to ¼ as derived above, nor is it equal to 1/3 as derived from the modern equation.

2.5 – "Perpetually revived and enlivened"

After a body collides with another, the initial motion is impeded. Beeckman's mathematical studies of collision investigated the amount of motion that is "removed," "destroyed," or "carried off,"[127] in each impact. This was done in service to the question he had first posed in his very first study: "why, may I ask, then is universal rest not brought about?" Beeckman maintained this focus in all of his studies of collision beginning in the winter of 1618, again in the spring of 1620, the fall of 1629, and the fall of 1634.

Some historians have claimed that Beeckman's work on collision is a source of Descartes's notion of the "conservation of the quantity of motion."[128] Beeckman used the

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[126] Beeckman, *Journal* 2:46. *Si autem inaequalia sint corpora, aequalis verò celeritas, auferetur minus corpus à majore et tum minus considerabitur ut quiescens. Si igitur residuum majoris corporis sit aequale minori corpori, movebitur utrumque dimidio pristinae celeritatis, sicut antè audivimus.*

[127] *Auferre* – to remove (Beeckman, *Journal* 2:45); *perdere* – to destroy (Beeckman, *Journal* 1:266); *rapere* – to carry off (ibid).

[128] Richard Arthur, "Beeckman, Descartes and the Force of Motion," *Journal of the History of Philosophy* 45 (2007): 3. Cornelius de Waard, the editor of Beeckman's *Journal* also made a similar claim see *Journal* 2:265n "Dans la suite de cette note Beeckman va déduire les lois du choc des corps mous au moyen du

inverse proportion of speeds and bodies before and after collision. This proportion describes a balance in equilibrium, so at least implicitly Beeckman may have presumed that the product of speed and body remains the same before and after collision. However, nowhere did Beeckman assert such a claim. Beeckman was concerned primarily with what he described as the destruction of motion. In all of the quantitative scenarios of collision, described above in section 2.4, the focus is on the amount of motion that is removed in each collision.

His study in 1618 is introduced with the heading, "in what way motion in a vacuum is impeded by means of collision," and the study was concluded with the statement, "Motion in a vacuum never increases, but decreases."[129] The emphasis remains on the amount of motion that is "removed" when he returned to the topic in 1620.[130] The question that he posed in his first study, why is universal rest not brought about, is raised again in 1629. He claims that he would like to "render an account why everything in the universe does not in the end rest," but his studies of collision seem to show that motion must decrease. Even little bodies can stop larger bodies—in a passage contrary to Descartes's fourth rule in which a smaller body cannot move a larger, Beeckman notes that "one atom...will move the entire Earth... Thus the little [bodies] stop the larger [bodies]..." Nevertheless he continued to wonder how motion in a vacuum could be

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principe de la conservation de la quantité de mouvement. On ne voit aucune intervention de Descartes, quoiqu'il puisse avoir eu connaissance de ces lignes. Plus tard (1644) celui-ci publia ces lois, mais il les appliqua à des corps qu'il considérait comme durs." Also see *Journal* 3:129n "C'est à dire la quantité de mouvement (*mv*) se conserve. Un passage du *Monde* de Descartes, commencé à la fin de 1629 (cf. *Oeuvres*, t. XI (1909), pp. 41-43) peut faire supposer que ce théorème qui a joué un si grand rôle dans sa philosophie, lui était alors connu."

[129] Beeckman, *Journal* 1:265-7.

[130] Beeckman, *Journal* 2:45-7. Rather than discuss the "transfer" of motion, Beeckman consistently uses words such as *auferet*: "one [body] will remove ("carry away" *auferet*) the motion of the other."

increased.[131] This was still his primary concern in 1634.[132] He surmised that motion can take place in a vacuum, as long as something produces it. He provided examples of stones being launched in the air by boys using sticks, presumably as an example of motion increasing,[133] but then he immediately noted that two equal bodies meeting with equal speeds will stop. Moreover, he still faced the problem of explaining source of movement in the boys, and how it is possible that the motion in boys increases as they grow: "the arteries of boys frequently seem to pulse, as the meagerness of the corpuscles require of them. Yet hence forth it increases, becomes strong etc. Then the pulse is very good, as I said before."[134] The world is full of what appears to be instances of motion increasing, but Beeckman's mathematical studies of collision show that motion must decrease. In 1634 he was still asking the same question: "why doesn't everything in the end rest? Can it be that fire has an influence to some extent? Or can it be that the very same action as reflection in a vacuum is to such a degree unknown?"[135]

In Beeckman's first study, he wrote: "so with this [the mathematical account of collision] in place, motion in a vacuum can never be intelligible. Faster motions, can be drawn to an end."[136] According to his mathematical account of collision, universal rest should be produced. But it is not. One observes that motion not only seems to persist but

[131] Beeckman, *Journal* 3:128-9.
[132] Beeckman, *Journal* 3:363-4.
[133] Beeckman, *Journal* 3:363. "[This might occur] by means of that method which boys move a ball upwards, quickly inflicting it with a large beam of wood. Let AB be a line. About C has been set a stapedi or stone; a ball is set at the extremity towards B. A boy strikes the other extremity at A. The ball quickly ascends as the hitting-stick is moved downwards."
[134] Ibid.
[135] Beeckman, *Journal* 3:364. *Cur non igitur tandem omnia quiescunt? An ignis hic aliquid potest? Aut an ejus actio tam est ignota quam reflectio in vacuo?*
[136] Beeckman, *Journal* 1:266. *His ita positis nunquam motus in vacuo potest intelligi ad celeriorem motum vergere*.

it seems in some instances to increase. In the absence of a satisfying rational account,[137] he appealed to divine intervention:

> Whence it follows that only *Deum Opt. Max.* is able to conserve motion with the largest bodies about to be moved by means of the largest speed, whose remainders, one after another, the forever contemplating being perpetually revives and enlivens [*perpetuo resuscitant et vivificant*].[138]

After a body collides with another, the initial motion is impeded. Part of the motion is "removed," which leaves a remainder [*reliquum*] of that motion. According to Beeckman, it is this remainder of motion that God "revives" and "enlivens." This notion of "conservation" is quite different from that of Descartes and subsequent natural philosophers. For Descartes, the principle of the "conservation of quantity of motion" would be prior to laws of motion and the rules of collision. Rooted in the immutable action of God, the constancy of the quantity of motion would be the “universal and

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[137] In 1618 Beeckman wrote an essay on faith, reason, theology, and philosophy, in which he discussed the limits of reason, and considered how those limits fit into his taxonomy of "what comes into our thinking." Many of these same ideas were presented again in the speech he gave when receiving his medical degree from Caen, France later that year, which can be found in Beeckman, *Journal* 4:40-5. An example of the limits of reason is the attempt to understand infinity and eternity. He claims that proportions are absurd regarding the infinite; and "therefore we do not comprehend it. ... Although we may *seem* to understand infinity with the mind, it will slip away." Our thinking is circumscribed and limited to the finite. See Beeckman, *Journal* 1:131. However, according to Beeckman’s view, there are other ways in which things can "enter our thinking." For example, "we believe with faith," *i.e.* through *conscientia*. In Beeckman’s speech upon receiving his medical degree later in 1618, he explained what is meant by this term. "The mind moves around some divine things and simply believes and agrees, not because it understands that, but by some supernatural impulse, which part perhaps is not ineptly called the *conscientia*." See Beeckman, *Journal* 4:40-5. But this is contrary to the knowledge established by reason. It is the role of reason and philosophy to seek out the extremities. However, not everything that "comes into our thinking" by means of an appeal to divinity is in the realm of *conscientia*. According to Beeckman, the knowledge of some of God's actions are taken to be the very epitome of reason: "Truly, what is more divine than the revolutions of the wandering stars [*erraticarum*] liable to no error? What is their motor? Who is the author? Who is their ruler [*praeses*]? Can it not be the most certain deduction that we assent to that some intelligence so eloquently joined [*coaptavit*] these? By no means differently do we certainly understand that a helmsman is holding the helm of the ship, when the ship is directed straight to the port?" See *Journal* 4:40. Several years before his dissertation speech, he also expressed the rationality of design in his atomic nature through an analogy to architecture: "Without a doubt as architects prepared the first houses: door, window, post, beam, covering, stone, as King Solomon constructs the first beginning of the temple ... so the God of nature fashioned the natural beginnings, which mutually come together to each other, they correspond as keys and pores, as definite things hence are born: stones, trees, animals..." See *Journal* 1:23.

[138] Beeckman, *Journal* 1:266-7

primary [...] cause of all the movements in the world."[139] In 1634 Beeckman concluded that, "therefore motion created once by God is conserved forever no less than material itself." [140] But, for Beeckman, God intervenes contrary to the consequences of his mathematical account of collision to revive and enliven the remainders of motion upon each collision. Beeckman's notion of the conservation of motion is a *deus ex machina*

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Beeckman explained various natural phenomena by means of the motion, arrangement, and shape of corpuscles behaving according to the principles of mechanics. This "physico-mathematics," in which collision was the *explanans*, was fruitful for both Beeckman and his colleague René Descartes. However, the collision of atoms as an *explanandum* proved to be a longstanding puzzle for Beeckman. His mathematical studies of the collision of the smallest bodies seem to show that motion was impeded upon each impact. Accordingly, all motion should come to universal rest. But nature appears to be continually in motion, and there are instances in which motion increases. Each of Beeckman's mathematical studies of collision are framed by this question and related concerns: in what manner is motion impeded by collision, why is universal rest not brought about, and is it possible for motion to increase? His mathematical investigation of collision was not a positive account of the rules of collision, or the expression of a principle of conservation. Rather, they are an investigation of the amount of motion that is "removed," "destroyed," or "carried off" in each collision. He appealed

[139] AT VIII 61. *Principia* II 36. Translation quoted in Valentine Rodger Miller and Reese P. Miller, trans., *Principles of Philosophy* (Dordrecht: Reidel, 1983) 58.
[140] Beeckman, *Journal* 3:369.

to divine intervention to explain why, contrary to his mathematical studies, motion does not come to universal rest. According to Beeckman, God continually revives and enlivens the remainders of motion upon each collision.

For Beeckman, mathematics and mechanics were primarily about the relationships between quantities. The mathematics Beeckman used to study collision was centered on the proportions of machines. Specifically, he relied on the pseudo-Aristotelian account of the balance in his investigation of collision. This framework, in which the speeds before and after collision are inversely proportional to the sizes of the bodies before and after collision (after collision the size is the aggregate of both bodies), did not provide a unified strategy for every scenario of collision. Nevertheless, it was influential, and would be taken up in Descartes's early account of collision, as we will see in the next chapter. And the balance would come to have a central role in each account of collision covered in this dissertation.

Chapter 3
Descartes on material contact: changing the rules of collision

I do not promise you to set out here exact demonstrations of all the things I will say. It will be enough for me to open to you the path by which you will be able to find them yourselves, whenever you take the trouble to look for them. Most minds lose interest when one makes things too easy for them. And to compose here a setting that pleases you, I must employ shadow as well as bright colors. Thus I will be content to pursue the description I have begun, as if having no other design than to tell you a Fable.

—from Descartes's 7th chapter, "On the Laws of Nature of this New World," of *The World*

Chapter 3 outline

Section 1
Introduction

Descartes's rules of collision were perceived as problematic at best by his contemporaries. They were not, however, insignificant. His work was pivotal. Descartes’s conservation principle, the "impact law," and the 7 rules of collision in the *Principles of Philosophy* framed a new vision of the world. It set the topic—the mathematical study of collision—as an area of investigation, and inspired the hopes of the next generation of thinkers who sought to explain all the changes of nature in terms of quantitative rules of collision. They accepted the general manner of posing the problem, but were critical of Descartes's results and the means of arriving at those results.

Descartes’s work is a turning point. Descartes’s older colleague, Isaac Beeckman, who also wrote on collision, but who did not publish on the topic, had used mechanical principles similar to those that Descartes would use, and Descartes’s early calculations of collision are akin to those found in Beeckman’s *Journal*. However, as we have seen in the previous chapter, Beeckman kept returning to the topic of the mathematical study of collision because it seemed to present a problem with his corpuscularian matter theory. Beeckman’s study of collision led him to the conclusion that motion should be reduced upon each impact of the smallest parts of matter. These parts of matter must be perfectly hard and thus, according to Beeckman, there is no rebound. And yet motion does not come to a halt, nor does every corpuscle attach to every other corpuscle after collision, resulting in a motionless block of matter. This puzzled Beeckman. Ultimately he saved his corpuscularianism with a *deus ex machina*: God continually "enlivens" motion upon each collision, contrary to what Beeckman’s mathematics seemed to show. Although Beeckman mathematically analyzed the transfer of motion in collision, he did not have a mathematical analysis of rebound. As we will see, although Descartes's early views on collision used calculations similar to those of Beeckman, his later view of collision seems to have been expressly designed to have the means to calculate instances of rebound. Descartes also shifted the focus from Beeckman's concern with the apparent decrease in motion, to the persistence of—not motion—but what Descartes called the "quantity of motion."

The rule of collision that articulated the conditions of rebound in Descartes's *Principles of Philosophy*, rule 4, was almost immediately challenged and shown to be false by the young Christiaan Huygens. As we will see in the next chapter, Huygens used the analytical tools developed by Descartes himself to challenge Descartes's rules of collision.

Descartes's mathematical studies of collision are unique. He contended with the problem without two components that would later prove to be key to its solution. He did not use a form of mathematics capable of expressing a system of equations with two variables. And he did not make use of the pendulum, which would become an important object to empirically measure speeds before and after collision. Descartes has at times been cast as a rationalist who had a purely mathematical vision of nature and whose method included deducing physics from first principles. We find a different Descartes in his work on collision—someone using mechanical principles, common experience, and observations to justify his understanding of the forces involved in collision, and who at times appealed to what appear to be entirely qualitative notions. What is revealed in Descartes's studies of collision and its ultimate publication, is not the notes for the most optimal rationalist deduction, but rather someone who was actively contending with a new problem.

Descartes’s understanding of collision changed and developed. After a review of the status of Descartes's work on collision throughout the history of science, this chapter will establish that a significant change did in fact occur in Descartes's understanding of collision. Emphasizing the development of Descartes’s view of collision shines new light on several issues of contention in Descartes’s physics, provides a unique perspective of Descartes’s intellectual activity, and begins to explain some of the more perplexing features of Descartes’s rules of collision in their final form. The chapter *describes* but does not *explain* why Descartes's view changed. The crucial change took place in 1639, which is also when Johannes Marcus Marci's *De proportione motu* was published in Prague. Suggestively, Descartes's later view shares significant similarities with Marci. However, more research is required to establish influence. Notably this chapter emphasizes the crucial change in 1639, rather than the relatively minor changes, previously documented by Garber, after 1644 among the Latin and French editions of the *Principles* and the letter to Clerselier.

With particular attention to the historicity of mathematics, the rules of collision in Descartes's mature work will *not* be reconstructed in modern mathematics. Descartes famously contributed to the emerging field of analytic geometry, by using symbolic algebra as an analytic method for solving geometrical problems. However, he did not use symbolic algebra anywhere in his account of collision, or in his physics generally. With attention to Descartes's own mathematical expressions, the focus in the rules of collision will be on the contest between forces in collision, which is described by Descartes's impact law. Doing so, Descartes's rules of collision will be organized in a new way, which uncovers an underlying pattern running throughout the rules. This new interpretation of Descartes's rules of collision not only stays true to the texts themselves, but also resolves several perennial interpretive difficulties that historians have faced.

Section 2
A brief history of the histories of Descartes and collision, 1847—present

René Descartes is a complex and important figure. The significance of his work, particularly on collision, has had a tumultuous place in the histories of science. His ideas have been judged to be an outright obstacle to the development of science, but also heralded as an emblem of perhaps the most important trend in the scientific revolution. His rules of collision have been ignored as entirely insignificant, analyzed with intense care, rejected as erroneous, and lamented as underdeveloped. Whether he was an obstacle or visionary, a genius or a misguided armchair philosopher, he continues to hold a primary place in the history of early modern science.

Descartes's historic stature merits an extended historiographical essay. Such an essay can be found in appendix 1. In what follows, I touch on some of the themes that are directly relevant to the rules of collision, such as the rise and fall of Descartes's place in the narrative of the mathematization of nature, the changing status of the rules of collision, and the common criticisms of Descartes's physics. The section will conclude by positioning this chapter within the tradition of histories of the science of Descartes.

2.1 – The rise and fall of Descartes's place in the narrative of *mathematization*

In the first half of the twentieth century, E. A. Burtt[141] and E. J. Dijksterhuis[142] argued that the *mathematization* of nature was one of the key developments leading to the emergence of modern science. They also claimed that Descartes significantly defended the notions that (a) the structure of the world is mathematical and that (b) this structure should be understood using a universal mathematical method. This theme was absent in earlier histories of science and mechanics by William Whewell[143] and Ernst Mach.[144] For Whewell, the essence of science should not be considered to be mathematical at all.[145] Mach, using modern mathematical reconstructions, was interested in showing that the deeply held, but sometimes-confused, fundamental principles in the history of mechanics could be derived from clearer modern principles.[146] Although the *mathematization of nature* has been influential since Burtt and Dijksterhuis, Descartes's place in it was almost immediately criticized. According to Alexandre Koyré, for example, Galileo should receive credit rather than Descartes.[147] Moreover, as Paul Mouy,[148] Koyré,[149] and many others have alleged, although matter may be identified with extension for Descartes, the details of his physics are qualitative and not quantitative. Additionally, the narrative crafted by Dijksterhuis and Burtt relies on claims that have since proven to be

[141] E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, New York, 1924. Revised Edition 1932 (Garden City: Doubleday & Co., Inc., 1954).
[142] E. J. Dijksterhuis, *Mechanization of the World Picture*: *Pythagoras to Newton*, Amsterdam, 1950, trans. C. Dikshoorn and reprinted (Princeton: Princeton University Press, 1986).
[143] William Whewell, *The History of the Inductive Sciences, from the Earliest to the Present Time*, vol. 2 (London: Parker, 1857). William Whewell, *The Philosophy of the Inductive Sciences, Founded upon their History*, vol. 1 (London: Parker, 1847).
[144] Ernst Mach, *The Science of Mechanics: a critical and historical account of its development* (Chicago: Open Court, 1893). Ernst Mach, *The History and Root of the Principle of the Conservation of Energy* (Chicago: Open Court, 1909).
[145] Whewell, *Philosophy of the Inductive Sciences,* 162-3.
[146] Mach, *The Science of Mechanics*, 305-31.
[147] Alexandre Koyré, *Galileo Studies* (Atlantic Highlands: Humanities Press, 1978), 201.
[148] Paul Mouy, *Le Développement de la Physique Cartésienne, 1646-1712* (Paris: Vrin, 1934), 144.
[149] Koyré, *Galileo Studies*, 90.

controversial at best, such as the significance and enduring role of the *mathesis universalis*, described by Descartes in the early and incomplete work the *Regulae ad Directionem Ingenii.*[150] Although historians gave Descartes an important position in the early statements of the thesis of the mathematization of nature, later historians quickly challenged his physics as not being mathematical at all with some claiming that his published physics is as qualitative as that of Aristotle. Moreover, there has been a shift from a narrower view of Descartes's physics to a view of a more far-reaching Cartesian "natural philosophy," of which Descartes's physics is only one part.[151]

2.2 – The status of the rules of collision: from insignificant to incomplete

The rules of collision (in general and Descartes's in particular) were notably insignificant in the histories of sciences by Whewell and Mach. Mach makes no mention of them in the history of the conservation of work, although he does mention principles developed prior to the rules of collision as imperfectly articulating the notion of the conservation principle. In his history of mechanics, the rules of collision are mentioned only in so far as they were an opportunity to enunciate prior principles of mechanics. Whewell claimed that they expressed a confused connection to momentum and the true "third law of nature."[152]

More so than their relation to the laws of nature or momentum, the rules of collision became significant in histories of science when connected to various presumed

[150] Chikara Sasaki, *Descartes's Mathematical Thought* (Boston: Kluwer Academic Publishers, 2003), 202. John Schuster, "Descartes' *Mathesis Universalis: 1619-28*," in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: The Harvester Press, 1980), 41-96.
[151] Stephen Gaukroger, John Schuster, and John Sutton, eds., *Descartes' Natural Philosophy* (New York: Routledge, 2000).
[152] Whewell, *History of the Inductive Sciences*, 56.

early modern trends: *mathematization*, *mechanism* and the *mechanical philosophy* (Burtt, Dijksterhuis, Aiton,[153] Westfall[154]). However, the Cartesian rules of collision bear a debated connection to *mathematization* since Descartes did not clearly quantify notions such as the "quantity of motion," "the moving force," and "the force of resistance." He did, however, "calculate" the resulting speeds of bodies in his rules of collision, although significantly he did *not* do so with algebraic calculations. The Cartesian rules bear a perplexing connection to *mechanism* since the bodies involved in collision are described in situations that cannot be realized in Descartes's system of the world—in the rules they are isolated from all other bodies, whereas Descartes's system of the world insists on a plenum. And as several historians have noted, in the plenum everything depends on everything else, and thus the system is much too complex to be expressed in mathematical form. Although Descartes's rules have always been considered to be problematic, if not incorrect outright, in the context of the *mechanical philosophy*, they proved to be a fruitful starting place for later work, namely the work of Christiaan Huygens, Christopher Wren, and other members of the Royal Society. They captured the enduring hope of describing all the "vicissitudes" of the world in terms of matter in motion. The quantitative rules of collision were to be the new alphabet of the language of nature.

Although the rules of collision would become incredibly important, Costabel, Gabbey, and Garber have suggested that his rules of collision may not have been all that

[153] E. J. Aiton, "The Vortex Theory of the Planetary Motions," *Annals of Science: a quarterly review of the history of science since the renaissance*, 13 (1957): 249-264. E. J. Aiton, *The Vortex Theory of Planetary Motions* (New York: American Elsevier Inc., 1972).
[154] Richard Westfall, *The Construction of Modern Science: Mechanisms and Mechanics* (Cambridge: Cambridge University Press, 1977).

significant for Descartes himself. They claim that the rules were never fully developed, and were hastily attached to the *Principia Philosophia* at a late date.[155]

2.3 – Criticisms of Descartes's physics

Mach criticized Descartes's apparent neglect of using experience to test his ideas, and thought that Descartes was generally overconfident. He criticized Descartes's conceptions (such as quantity of motion) for being indistinct, and disapproved of what he took to be Descartes's method: asserting that there are "self evident" *a priori* truths from which to deduce physics. Both Whewell and Mach agreed that, in general, Descartes's work in physics pales in comparison to Galileo's, and according to Koyré it pales even with respect to the program of the mathematization of nature. Ultimately, according to Mach at least, Descartes's work in physics was insignificant and would be completely effaced.[156]

Whewell judged Descartes's laws of motion to be false in substance. Some, just as Paul Tannery,[157] have made exceptions for the first two laws, but the third law (the impact law) in particular has been deemed incorrect. The key problem, as Dijksterhuis and Westfall have indicated, is that Descartes did not understand velocity and momentum as vector quantities. However, others, such as Aiton, have claimed that Descartes's primary mistake was that he thought impact was the only cause of changes in motion.

[155] Pierre Costabel, "Essai critique sur quelques concepts de la mécanique cartésienne," *Archives Internationales d'Histoire des Sciences* 20 (1967): 235-52. Alan Gabbey, "Force and Inertia in the Seventeenth Century: Descartes and Newton," in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: Harvester Press, 1980), 265. Daniel Garber, *Descartes' metaphysical physics* (Chicago: University of Chicago Press, 1992), 231, 242, 252.
[156] Mach, *The Science of Mechanics*, 250, 273-4.
[157] Paul Tannery, "Sur les règles du choc des corps d'après Descartes," in AT IX part 2, 327-30.

Similarly, historians since at least Whewell have claimed that his rules of mutual impact of bodies were erroneously given. For many, such as Diederik Korteweg[158] and Dijksterhuis, the rules were considered to be relevant only insofar as they were *corrected* by Huygens. Even if the rules of impact are understood "on their own terms," Descartes has been criticized, by Aiton for example, for failing to adequately explain the connection between the third law and the rules of collision, particularly those rules which involve equal forces, *i.e.* those that seemingly cannot be derived from the third law, which requires a contest between forces. And, as many have noted, if Descartes held a relative notion of motion, as has been the received view,[159] then Descartes's rules of collision are inconsistent, *e.g.* rule 4 and rule 5.

Whewell has observed that, generally, those in the 17th century conflated the principles of statics and dynamics, and attempted to use static notions such as weight to measure dynamic forces such the force of impact. According to Westfall, the notion of *force* was particularly problematic in the mechanical philosophy; Descartes and the mechanical philosophy were an obstacle to the successful synthesis of the trend of finding mechanisms underlying natural phenomena and the trend of mathematical platonic-pythagoreanism, whereas Newton successfully united these trends. Descartes relied on a notion of the force of a moving body and had no explanation for force as a cause of motion. Alan Gabbey has also criticized Descartes's notion of force, but

[158] The applied mathematician and historian of mathematics, Diederik Korteweg, was the lead editor of the *Oeuvres Complètes de Christiaan Huygens* from 1911 through 1927. Volume 16 includes Huygens's work on collision, as well as substantial notes on Descartes's rules of collision (HOC 16: 4-5n). The volume had been started by Korteweg, who was responsible for pages 1-186, 202-212, but, like Tannery and the AT, his death prevented him from completing it. J. A. Vollgraff oversaw its completion as well as the remaining volumes of the project. Notably, E. J. Dijksterhuis also worked on volume 16 (pages 344-349, 392-412, 463-469). It is Huygens's work on musicology in the first part of volume 20 to which Dijksterhuis contributed the most. See HOC 22: 816.

[159] Garber has notably challenged the recieved view of Descartes's notion of motion in *Descartes's Metaphysical Physics*.

primarily for failing to clearly explain its ontological status. Unlike Westfall, Gabbey has claimed that Descartes's notion of force (particularly the force of resistance of a body at rest) anticipates Newton.[160] In doing so, Gabbey has relied on an important, although mistaken, interpretation of the force of resistance, which has roots in Aiton's work on the topic.

For some commentators, such as Burtt and Koyré, Descartes's explanations of phenomena by material contact in vortices indicate a wrong turn generally. His physics was consequently too complex to express in mathematical form. Despite Descartes's presumed mathematical vision of nature and mathematical method to understand nature, Dijksterhuis notes that there are nearly no instances of algebraic calculations, which he expected to find since he believed that Descartes's underlying method was akin to Descartes's *mathesis universalis* (which bears important connections to symbolic algebra). As Mouy, Koyré, and Garber have claimed, Descartes's published physics may seem mathematical, but on closer inspection it is largely qualitative, "a mathematical physics without mathematics."[161] Garber further identifies several deep flaws with the details of Descartes's physics: one notable instance is that in Descartes's plenum, motion and body are circularly defined (a body is just that part of matter that moves together with respect to its surroundings, and motion is the separation of one part of matter from the

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[160] Gabbey, "Force and inertia," 269-72.
[161] Mouy, *Développement*, 144. Koyré, *Galileo Studies*, 90. According to Mouy, "La physique cartésienne est une physique mathématique sans mathématiques. C'est une géométrie concrète, ce n'est pas une géométrie analytique, une algèbra de l'univers." This was also noted by Koyré, "It is well known that Descartes' physics, as it is set out for us in the *Principes* no longer contains mathematically expressible las. It is, in fact, no more mathematical than that of Aristotle." Also see Daniel Garber, "A different Descartes: Descartes and the programme for a mathematical physics in his correspondence," in *Descartes' Natural Philosophy*, ed. Stephen Gaukroger, John Schuster, and John Sutton (New York: Routledge, 2000) 114. "The physics of the *Principia* is all words."

matter immediately neighboring it). This poses several problems, not the least being the individuation of bodies—a body at rest disappears into its surrounding.[162]

2.4 – This chapter's place in the historiography

Past historians such as Whewell, Mach, and Tannery have thoroughly noted the shortcomings of Descartes's physics using the standard of either the "truth of the laws of nature" or the principles of mechanics accepted in their day to show where Descartes erred. The merits of Descartes's ideas have been measured by other standards as well. For Burtt, Dijksterhuis, and Westfall, the standard was not the truth of Descartes's work, but rather the influence it had on shaping the view of the world—the metaphysical presuppositions of moderns (Burtt), a mathematized nature (Dijksterhuis), or the mechanical philosophy (Westfall). I am less interested in measuring the extent to which Descartes's ideas depart from some chosen standard (whether it is modern science or a broader historical trend), than I am interested in making sense of Descartes's thought-process. In this way my study is similar to those of Aiton, Garber, and Gaukroger, who used neither the standard of modern science, nor that of an ensuing "world picture," but rather a notion that they had of Descartes himself—for Garber this was Descartes's systematic project and for Gaukroger it was the interior development of Descartes.[163] However, even in these studies, modern mathematics has been used as an interpretive tool to reconstruct Descartes's ideas. This has imported notions that are foreign to Descartes's thinking, and has produced unnecessary interpretive difficulties. Attention to the historical mathematics is particularly important in the case of theories of collision,

[162] Garber, *Descartes' Metaphysical Physics*, 175-181.
[163] See appendix 1 for a discussion of Garber's and Gaukroger's contrasting explanations of some of the more contentious issues in Descartes's work.

because mathematics is deeply interwoven with the conceptual analysis of the problem, and, as I will argue in chapters 4 and 5, it was in the very context of collision that physical applications of symbolic algebra were developing.

In the case of Huygens, I pay close attention to the role of algebra, particularly the role of signs, in the development of his theory of collision, as we will see in the next chapter. Most have taken these concepts for granted, and have used modern algebraic expressions as an interpretive tool. But these very concepts were in the process of being born in the early modern period—specifically in the context of collision. Huygens used algebra to investigate collision and to criticize Descartes's rules, but struggled to make sense of the solutions produced by the equations. The *development* of algebra (in its physical applications) is key for the interpretation of Huygens's achievements, as well as the interpretation of the development of theories of collision in general. I urge more restraint in the use of modern mathematics, particularly symbolic algebra, as an interpretive tool in histories of early modern theories of collision. Regarding Descartes, closer attention is given to his own way of framing the problem of collision. Mathematics is not a neutral system, and reconstructing Descartes's theory of collision using mathematical ideas that Descartes did not use, distorts the theory. It has also led to a mistaken interpretation of a fundamental aspect of his theory, which has subsequently produced unnecessary interpretive difficulties. Rather, I focus on Descartes's own expression of collision, specifically the contest of forces. Doing so has uncovered a significant shift in Descartes's own understanding of collision before and after the *Principles of Philosophy.* It has also uncovered a new organization of the rules, which has led to a new interpretation of Descartes's theory of collision. This new interpretation stays closer to the text and resolves perennial interpretive difficulties.

Although Descartes's rules of collision are problematic, and although they are difficult, if not impossible, to reconcile with his definitions of motion, they are not as incomplete and problematic as recent historians have suggested. As the rules appear in the *Principles of Philosophy*, they set the problem of a quantitative investigation of collision. The underlying pattern in the rules will make clear that Descartes used a systematic and consistent analytic method to solve this new problem (specifically, to solve what was new to his "later view" of collision—the conditions of rebound). Although Descartes did so much to develop symbolic algebra as an analytic method in geometry, he never used symbolic algebra in the rules of collision. Nevertheless, Descartes's rules of collision have an important place in the "mathematization of nature." The rules of collision are quantitative—one of the few aspects of his physics that actually is. And the next generation of thinkers will bring Descartes's symbolic algebra together with the new problem of the quantitative rules of collision, with devastating consequences for Descartes and incredibly fruitful results for both the study of collision and the development of algebra.

Section 3
Overview of Descartes's projects in *The World* and the *Principles of Philosophy*

Descartes worked on two major texts, *The World* and *Principles of Philosophy* while holding his early and late views on collision respectively. In *The World,* Descartes constructed an imaginary world from mechanical principles in order to showcase his account of light.[164] The imagined physical world is shown to be a plenum composed of three elements, distinguished primarily by their size. The elements correspond to (1) sources of light, made of the smallest bits of matter, (2) the heavens, or that which transmits light, made of the medium-sized bits of matter, and (3) the Earth, or that which reflects light, made of the largest bits of matter. In order to present his theory of light, he first constructs a multiple heliocentric cosmology. Beginning with a state of chaos, he explains how the three elements are formed and how collections of the first element (the source of light) accumulate in the center of each of the indefinitely many vortices. After much work and several chapters, he shows that light corresponds to linear pressure extending outward from the center of the swirling vortices in the plenum. Ultimately, his explanations are supposed to rely only on the differently sized and shaped parts of matter moving in contact with each other—grinding, glomming together, and knocking off each other's edges—all according to three laws of nature and a principle of the conservation of quantity of motion. In 1633, upon the condemnation of Galileo for his argument that the Earth moves, Descartes broke off work on *The World*.[165]

The Principles of Philosophy was modeled on Scholastic textbooks in its style of presentation, terminology, and scope of topics.[166] The published work is in four parts. Parts II and III—The Principles of Material Objects, and The Visible Universe—offer content similar to *The World,* but reworked and presented in a new way, with some significant differences regarding collision. Again, Descartes constructs the world from

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[164] AT XI 31, ll. 22-25. "For a short time, then, allow your thought to wander beyond this world to view another, wholly new one, which I shall cause to unfold before it in imaginary places." Translation quoted in Michael S. Mahoney, trans., *The World or Treatise on Light*, accessed January 25, 2015, http://www.princeton.edu/~hos/mike/texts/descartes/world/worldfr.htm.

[165] AT I 270-3, 284-91. Descartes to Mersenne, November 1633. Descartes to Mersenne, April 1634. Stephen Gaukroger, *Descartes' System of Natural Philosophy* (New York: Cambridge University Press, 2002), 20-1. Also see Garber, *Descartes' Metaphysical Physics*, 182.

[166] Gaukroger, *Descartes' System*, 32-63.

mechanical principles. However, unlike *The World* Descartes first presents The Principles of Human Knowledge in Part I, which provide a justification and foundation for his natural philosophy. In it, Descartes attempts to clear away the readers' learned prejudices, and then uses the criteria of "clear and distinct ideas" to build his metaphysical categories from principles that anyone, even those versed in scholastic philosophy, should accept. Or at least he seems to hope as much. Generally, the material from *The World* found in sections II and III of the *Principia* is presented in a revised and expanded manner in light of the categories and commitments of Descartes's metaphysics.

3.1 – Collision as *explanans*: material contact in Descartes's natural philosophy

Toward the end of the *Principles of Philosophy*, part IV "Of the Earth," and thus also the end of the work itself, Descartes provides a set of sections that summarize his general view.[167] In doing so he summarizes the overarching principles he has used in his *natural philosophy*. The way in which he describes collision here is notable. First, he deftly claims in section 200 that his principles are nothing new. They are so commonplace (and old, obvious, and unoriginal) that no one has ever not accepted them.[168] These principles are (1) that bodies have figures, motions, and sizes; and (2) that using "the laws of Mechanics (which are confirmed by certain and daily experiences)" one can determine "what ought to follow from the collision of these bodies."[169]

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[167] In these last sections Descartes also distinguishes his views from those of others that the reader might be tempted to conflate. Particularly, Descartes distinguishes his views from the atomists.

[168] AT VIII 323. *Principia* IV 200. "[...] I have used, for this purpose, absolutely no principle which was not accepted by Aristotle and by all other Philosophers of all periods: so that this Philosophy is not new, but the oldest and most commonplace of all." Translation quoted in Valentine Rodger Miller and Reese P. Miller, trans., *Principles of Philosophy* (Dordrecht: Reidel, 1983) 283.

[169] AT VIII 323. *Principia* IV 200. *"Nempe figuras & motus & magnitudines corporum consideravi, atque secundum leges Mechanicœ, certis & quotidianis experimentis confirmatas, quidnam ex istorum corporum mutuo concursu sequi debeat, examinavi.*" Translation by Miller, *Principles*, 283.

> Yet who ever doubted that bodies are moved, and are moved variously according to their various sizes and figures; or that as a result of the collision of these bodies, the larger ones are divided into many smaller ones, and change their figures? We do not observe this through only one sense, but through several: through sight, touch, and hearing [*sic*]; and we also {very} distinctly imagine and {clearly} understand this.[170]

Although many would have been committed to a great many more principles than just these two, Descartes's point is that these are so basic that no one would deny them.

What is of interest in the context of this chapter is that collision is mentioned in (2), and that what is of importance regarding collision is that larger bodies are *divided* into many smaller ones and change their figure. In the context of his general summary, it is this that Descartes names as commonplace—*not* that a body moving to meet another of some size will transfer some amount of its motion to the other. What is observed (and obvious to all our senses and understanding) is motion, as well as bodies *dividing* due to collision.[171] The notion of collision here is *not* bits of matter rebounding off each other like billiard balls. This is in keeping with the kinds of *explanations* he presents of

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[170] AT VIII 323. *Principia* IV 200. Translation by Miller, *Principles*, 283.

[171] Throughout Descartes's early and late natural philosophy, bodies interact through material contact in three general of ways. Although the second and third can likely be reduced to the first, it is instructive to make the tripartite distinction. (1) They move past each other in vortices. This is almost trivially true since a solid body is just a collection of parts of matter, which are generally at rest with respect to each other, whereas the parts of matter surrounding the collection are not at rest with respect to it. (2) Bodies also interact by pushing each other. This includes his explanation of the weight of terrestrial phenomena, and the motion of planets being "carried along" by the heavenly fluid. See AT VIII 92. *Principia* III 30. His explanation of light is also a form of "pushing" – as the "pressure" produced by the centrifugal determination (but not movement) of parts of matter outward from a vortex. See AT VIII 108-135. *Principia* III 55, "What light is," and the subsequent explanations through section 80. In the *Principia*, magnetism too is explained as a kind of "pushing" – as the action of corkscrew-shaped parts of matter. See AT VIII 275-310. *Principia* IV 133-182. And (3), bodies interact through material contact by dividing and breaking each other apart. It is this last form of interaction (which, like the others, remains largely unchanged throughout his writings) that I would like to highlight. This is important because it shows that Descartes carried over many of the same explanations from his earlier works. Significantly, some of these explanations -- such as the action of fire (a fluid) on solids -- include smaller bodies moving larger bodies. In his early work this was entirely acceptable, as he had used that scenario to illustrate the meaning of "quantity of motion" and its conservation, as we will see. In his later work, however, this is much more complicated and appears to be in tension with the principles from which his natural philosophical explanations are intended to follow. See Aiton, "Vortex Theory," 254-255.

phenomena throughout his natural philosophy in parts III (Of the Visible Universe), and IV (Of the Earth) of the *Principles of Philosophy*, as well as *The World*.

Throughout Descartes's natural philosophy, phenomena are explained in terms of material contact. Collision is the *explanans*. In Descartes's "rules of collision" on the other hand, which are presented immediately after the laws of nature in the *Principles of Philosophy* (part II, sections 46 - 52), collision is the *explanandum*. Descartes presents his natural philosophy beginning with what he considers to be the simplest facets of nature—body and motion—and then constructs the world from these simplest facets. Similarly, he begins with the simplest principles—the immutable action of God, the conservation of the quantity of motion, and the first two laws of nature, which describe the persistence of motion or rest and the persistence of the direction of motion respectively. Throughout his presentation, the principles increase in complexity: the third law of nature describes, in general, what happens to the state of motion and rest (and the direction of motion) when a body impacts another. The rules of collision then quantitatively present the conditions of rebound and the transfer of motion for specific scenarios of impact between two bodies. Since the world described in Descartes's natural philosophy is a plenum, bodies are continually in contact, motion is continually being transferred, and the direction of motion is continually being affected. The rules of collision are an extension of his synthetic presentation of principles of increasing complexity. They describe the behavior of bodies, as if they were in scenarios in which only two bodies exist. Immediately after the rules of collision Descartes begins to describe solid and fluid bodies in terms of the plenum. Thereafter, phenomena are generally described in terms of material contact, with collision as the *explanans*.

3.2 – "Conservation," the Laws of Nature, and the "Impact Law"

According to Descartes, the laws of nature are such that given any initial set of conditions of matter and motion, the world that we know from our senses would eventually be produced. What Descartes's describes in *The World* "is a self-generating world that begins with chaos and shapes itself in an almost emergent way into a world that looks like the one we inhabit."[172] In the *Principles of Philosophy,* Descartes describes a different set of initial conditions in which matter and motion are in "proportion or order"[173] rather than chaos, and claims that the laws of nature would produce the same world. In the *Principles,* Descartes not only indicates that these are different initial conditions from those in *The World*, but he explicitly claims that the conditions in the *Principles* are false. They must be false because according to Descartes the account of the formation of the world in Genesis is true.[174] Nevertheless, no matter what the initial conditions are—ordered or chaotic (or biblical)—and even if what is

[172] Michael S. Mahoney, "The World of Descartes" (plenary address to the 7th Annual Conference of the Association for Core Texts and Courses, University of Notre Dame, 5-8 April 2001). Text quoted in Michael S. Mahoney, "The World of Descartes," accessed January 25, 2015, http://www.princeton.edu/~hos/Mahoney/talks/actc-descartes.html.

[173] AT VIII 101. *Principia* III 46. Descartes makes the following suppositions: (1) "[...] that God, in the beginning, divided all the matter of which He formed the visible world into parts as equal as possible and of medium size" (2) "[...] that He endowed them collectively with exactly that amount of motion which is still in the world at present" (3) "and, finally, that He caused them all to begin to move with equal force {in two different ways, that is}, each one separately around its own center, by which means they formed a fluid body, such as I judge the heaven to be; and also several together around certain other centers [...] so that [these] parts formed as many vortices [...] as there are now heavenly bodies in the world." Translation by Miller, *Principles*, 106-7.

[174] AT VIII 99-100. *Principia* III 45. "Indeed, in order to better explain natural things, I may even retrace their causes here to a stage earlier than any think they ever passed through. {For example}, I do not doubt that the world was created in the beginning with all the perfection, which it now possesses... The Christian faith teaches us this, and natural reason convinces us that this is true... But, nevertheless, just as for an understanding of the nature of plants or men it is better by far to consider how they can gradually grow from seeds than how they were created [entire] by God in the very beginning of the world; so, if we can devise some principles which are very simple and easy to know and by which we can demonstrate that the stars and the Earth, and indeed everything which we perceive in this visible world, could have sprung forth as if from certain seeds (even though we know that things did not happen that way); we shall in that way explain their nature much better than if we were merely to describe them as they are now, {or as we believe them to have been created}. And because I think I have discovered some principles of this kind, I shall here briefly expound them." Translation by Miller, *Principles*, 105.

described in the initial conditions is explicitly false, as long as the laws of nature are followed, the world that we know and perceive would be produced:

> [The] laws of nature are such that, even if we were to assume...the Chaos {of the poets, that is, a total confusion of all parts of the universe}; we could still demonstrate that, by these laws, this confusion must {gradually} be transformed into the order which is at present in the world. And...I formerly undertook to explain how this could have happened. [...] And it is almost impossible to imagine any arrangement from which we could not deduce, by these laws, the same effect; {since it must change continually, until it finally forms a world exactly similar to this one} [...].[175]

In both *The World* and *the Principles,* the three laws of motion are much the same: (a) each part of matter remains in the same state until collision with other bodies forces it to change,[176] (b) when a body transfers motion to another, it gives the other as much as it itself loses, otherwise it rebounds (*i.e.* the "impact law"),[177] and (c) each part

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[175] AT VIII 102-3. *Principia* III 47. Translation by Miller, *Principles*, 107-8.

[176] AT XI 38. *Le Monde*, chapter 7. "The first is that each individual part of matter always continues to remain in the same state unless collision with others constrains it to change that state. That is to say, if the part has some size, it will never become smaller unless others divide it; if it is round or square, it will never change that shape without others forcing it to do so; if it is stopped in some place, it will never depart from that place unless others chase it away; and if it has once begun to move, it will always continue with an equal force until others stop or retard it." Translation by Mahoney, *The World.*

AT VIII 62. *Principia* II 37. "The first law of nature: that each thing, as far as is in its power, always remains in the same state; and that consequently, when it is once moved, it always continues to move." Translation by Miller, *Principles*, 59.

[177] AT XI 41. *Le Monde*, chapter 7. "I suppose as a second rule that, when one of these bodies pushes another, it cannot give the other any motion except by losing as much of its own at the same time; nor can it take away from the other body's motion unless its own is increased by as much. [...] [T]he motion of a body is not retarded by collision with another in proportion to how much the latter resists it, but only in proportion to how much the latter's resistance is surmounted, and to the extent that, in obeying the law, it receives into itself the force of motion that the former surrenders." Translation by Mahoney, *The World*.

AT VIII 65. *Principia* II 40. "This is the third law of nature: when a moving body meets another, if it has less force to continue to move in a straight line than the other has to resist it, it is turned aside in another direction, retaining its quantity of motion and changing only the direction of that motion. If, however, it has more force; it moves the other body with it, and loses as much of its motion as it gives to that other." Translation by Miller, *Principles*, 61.

Garber has indicated that "there is little in the way of significant addition in the French edition of Pr II 40-45, where Descartes sets out law 3, its explication and its defense. [...] There is reason to believe that Descartes hardly looked at the French translation of those sections. As Pierre Costabel has pointed out, there are some errors of translation in those sections that are so glaring that Descartes could hardly have failed to notice them, had he but read them over with any care." Garber, *Descartes' Metaphysical Physics*, 248.

of matter tends to move in a straight line.[178] Both also state that the total quantity of motion in the world remains the same.[179] However, the order of the laws changes from *The World* to the *Principles*. In addition, the place of the "conservation principle" changes. In *The World,* the order is (a), (b), (c), and the conservation principle is stated after (a) and (b). The conservation principle is that from which (a) "parts of matter tend to remain in the same state," and (b) "the impact law," are supposed to follow. In the *Principles* (b) and (c) trade places. And the conservation principle is prior to all of the laws, being specifically called a "universal and primary [...] cause of all the movements in the world."[180] The laws are found in sections 37, 39, and 40, whereas the conservation principle is in section 36, the title of which states: "That God is the primary cause of motion; and that He always maintains an equal quantity of it in the universe." In contrast

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[178] AT XI 43-4. *Le Monde*, chapter 7. "I will add as a third rule that, when a body is moving, even if its motion most often takes place along a curved line and (as has been said above) can never take place along any line that is not in some way circular, nevertheless each of its individual parts tends always to continue its motion along a straight line. And thus their action, *i.e.* the inclination they have to move, is different from their motion." Translation by Mahoney, *The World.*
AT VIII 63. *Principia* II 39. "The second law of nature {which I observe} is: that each part of matter, considered individually, tends to continue its movement only along straight lines, and never along curved ones..." Translation by Miller, *Principles*, 60.
[179] AT XI 43. *Le Monde*, chapter 7. "Now it is the case that those two rules manifestly follow from this alone: that God is immutable and that, acting always in the same way, He always produces the same effect. For, supposing that He placed a certain quantity of motions in all matter in general at the first instant He created it, one must either avow that He always conserves as many of them there or not believe that He always acts in the same way." Also see AT XI 11. *Le Monde*, chapter 3. "I do not stop to seek the cause of their motion, for it is enough for me to think that they began to move as soon as the world began to exist. And that being the case, I find by my reasoning that it is impossible that their motions should ever cease or even that those motions should change in any way other than with regard to the subject in which they are present. That is to say, the virtue or power in a body to move itself can well pass wholly or partially to another body and thus no longer be in the first; but it cannot no longer exist in the world." Translation by Mahoney, *The World.*
AT VIII 61-2. *Principia* II 36. "As far as the general {and first} cause is concerned, it seems obvious to me that this is none other than God Himself, who, {being all-powerful} in the beginning created matter with both movement and rest; and now maintains in the sum total of matter, by His normal participation, the same quantity of motion and rest as He placed in it at that time. ... From this it follows that it is completely consistent with reason for us to think that, solely because God moved the parts of matter in diverse ways when He first created them, and still maintains all this matter exactly as it was at its creation, and subject to the same law as at that time; He also always maintains in it an equal quantity of motion." Translation by Miller, *Principles*, 58.
[180] AT VIII 61. *Principia* II 36. Translation by Miller, *Principles*, 58.

to God and the conservation of quantity of motion, the laws of nature are "the secondary and particular causes of the diverse movements which we notice in individual bodies." In *The World,* the conservation principle is also connected directly to God (which in the *Principles* is *the* universal and primary cause of motion), but Descartes does not make a distinction between particular causes and universal causes in *The World.*

As Daniel Garber has explained, the new order of the laws in the *Principles* is significant. The first two laws are "principles of persistence" that are closely related to the conservation principle. The first two laws concern the *persistence* of motion/rest and directionality respectively. And the conservation principle concerns the *persistence* of the total quantity of motion. The third law in the *Principia*, the impact law, is quite different from the first two. It is a "principle of *reconciliation*." "[I]n the Cartesian plenum it will *always* happen that the conditional principles of persistence will come into conflict. ... [Law 3] tells us what is to happen next, how the two incompatible conditionally persisting motions are to be reconciled with one another."[181] Thus, there is a shift not just in the order, but a shift in the significance of the "impact law" in the system.

Corresponding to the new significance of the "impact law" in Descartes's system is a new understanding of collision. This is seen most obviously in the change from his early position that a smaller body can move a larger body at rest, to his later position that a smaller body can never move a larger body at rest. The impact law states that when a moving body meets another, it meets a force of resistance. If the moving body cannot overcome this force of resistance, it changes direction (*i.e.* rebounds) retaining its motion. If the moving body does overcome this force of resistance, it transfers some of its motion

[181] Garber, *Descartes' Metaphysical Physics*, 203.

to the other. There is a contest between the force of resistance and the force of motion.[182] In Descartes's early view of collision, he does not explicitly describe under what conditions a body does not overcome the force of resistance. Although he states that it is possible, he provides no examples in which the force of resistance prevails, resulting in rebound. All of his examples of collision involve the transfer of motion. In his later view of collision, specifically in his rules of collision, Descartes explicitly presents the conditions in which the force of resistance is not overcome, resulting in rebound. Section 4 presents evidence of this fundamental change and outlines the major features of his early view on collision.

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[182] John Herival, *The Background to Newton's* Principia*: A Study of Newton's Dynamical Researches in the Years 1664-84* (Oxford: Clarendon Press, 1965) 49.

Section 4
Descartes's early account of collision: transfer of motion

Along with *The World*, a major source for our understanding of Descartes's early views on collision is his April 1639 letter to Florimond de Beaune. In the letter, Descartes presents his ideas on motion and the conservation of quantity of motion, as well as several of his ideas that will undergo development in his later work, including rebound, the transfer of motion, and the force of resistance. The views articulated in this letter indicate that Descartes had considered the topic of collision (the meeting and interaction of two bodies, where at least one is in motion) in a different manner from what is found in his later "mature works" such as the Latin and French editions of *The Principles of Philosophy* (1644, 1647) and his elaborations on the topic in his correspondence with Claude Clerselier (1645).[183] Evidence of this early view is not unique to the 1639 letter to de Beaune. It is supported by his other early writings including *The World* as well as his correspondence with Mersenne in the late 1630s. This indicates that Descartes did not hold one view on collision, a topic of importance to his physics and natural philosophy, as well as those of later natural philosophers and mathematicians. His views on the foundations of his physics were in flux throughout his career.

This section will characterize some of the main components of his early views on collision, including motion, the conservation of the "quantity of motion," and the conditions of the transfer of motion in the contest between the force of resistance and the moving force. Focusing on this fundamental change casts new light on the rules of collision in the *Principles of Philosophy*, his later account of collision. As we will see in section 5, this reveals a previously unnoticed pattern connecting the rules, which better

[183] See Appendix 2 for more information on the 1639 letter itself as well as an English translation.

captures Descartes's ideas and resolves several longstanding interpretive problems with the rules.

4.1 – Descartes to de Beaune on the quantity of motion

René Descartes's friend, Florimond de Beaune (1601-1652), who would write the introduction[184] to the first Latin edition of Descartes's *La Géométrie* (1649), asked Descartes in a 1639 letter about the measurement of speed, as well as "the nature of Weight," and what de Beaune called "Natural Inertia." Descartes had contacted de Beaune earlier that spring because he wanted to know if de Beaune could build an instrument that would grind hyperbolic lenses—an instrument of the kind that Descartes described in *La Dioptrique* (1637).[185] At the time, de Beaune had been working on his *Méchaniques*,[186] to which Descartes seems to have given high praise.[187] When he

[184] In addition to the introduction, *Novis briève* (*Notæ breves*), de Beaune wrote *De æquationum natura, constitutione* as well as *De limitibus æquationum*, both of which appear in the second Latin edition of Descartes's *Geometry* (1659).

[185] J. J. O'Connor and E. F. Robertson, "Florimond de Beaune," *MacTutor History of Mathematics*, accessed January 25, 2015, http://www-history.mcs.st-and.ac.uk/Biographies/De_Beaune.html. According to O'Connor's and Robertson's biography, "De Beaune became obsessed with the idea of making Descartes' machine to grind lenses and devoted his whole time to the project. Descartes knew that de Beaune was the only person who had the technical proficiency, a deep understanding of mathematics and a fascination with astronomy. However in January 1640, despite his expertise, de Beaune cut his hand badly on a piece of roughly shaped glass which he was trying to cut into a hyperbolic shape. When Descartes heard about the accident he seemed pleased that his scientific imagination went beyond what the best technician could make. He wrote to Christiaan Huygens's father: *Do you think I am saddened by this? On the contrary, I tell you that in the very failure of the hands of the best craftsman, I understand just how far my reasoning has reached.*"

[186] O'Connor and Robertson, "Forimond de Beaune." De Beaune's *Méchaniques* was never published. In addition, his *Dioptrique* was never published. To date, there are no known manuscript copies of these planned/completed works.

[187] AT II 542, 543. Descartes to [Mr. de Beaune], [30 April 1639]. "Your fashion of distinguishing diverse dimensions in motion, and representing them by lines, is without a doubt the best that can be. ... Your distinction of three lines of direction...is very methodical and useful. ... The Invention of your Curved Lines is very beautiful; and the reason that you give for the quadruple tension of a cord which makes the octave, is very ingenious and quite true." *Votre façon de distinguer diuerses dimensions dans les mouuemens, & de les representer par des lignes, est sans doute la meilleure qui puisse estre. [...] Vostre distinction des trois lignes de direction [...] est forst methodique & vtile. [...] L'Inuention de vos Lignes Courbes est tres belle; et la raison que vous donnez pour la tension quadruple d'une corde qui fait l'octaue, est tre-ingenieuse & tres-vraye.*

responded to de Beaune's questions on mechanics, he touched on the topics of natural inertia and weight. He claimed, however, that he could *not* comment on the measurement of speed:

> I would like to be capable of responding to what you want concerning your Mechanics; but even though all of my Physics is nothing other than Mechanics, however, I have never particularly examined questions that depend on measures of speed.[188]

In the letter to de Beaune, Descartes revealed why he had never examined questions that depend on the *measurement* of speed. He used two of the topics about which de Beaune had inquired to explain his position—speed and weight. First, Descartes claimed that there is a nuanced relationship between "quantity of motion" and speed, and maintained that the former "quantity" rather than the latter "speed" was more basic. He went on to claim that weight is the *effect* of something more fundamental, *i.e.* it is the effect of various motions of subtle matter in vortices between the Earth and the Moon. Presumably, the perceived and empirically measured characteristics of speeds and weights would not provide insight into these more fundamental quantities and motions. This is roughly the same position Descartes would keep in his later works on the topic. Another possible reason Descartes may not have examined questions that depend on the measurement of speed is that he had no practical way of measuring speed, at least in the context of collision. He does not mention ever using what would become the preferred method of later investigations on the topic—measuring the initial speed by the height from which a pendulum bob drops, and the speed after collision by the height to which the impacted pendulum bob is elevated and to which the initially moving bob returns. As

[188] AT II 542. Descartes to De Beaune, 30 April 1639. *Ie voudrois estre capable de répondre à ce que vous desirez touchant vos Mechaniques; mais encore que toute ma Physique ne soit autre chose que Mechanique, toutesfois ie n'ay iamais examiné particulierement les questions qui dépendent des mesures de la vitesse.*

we will see in chapters 4 and 5, the use of this method to measure speeds before and after impact may have been inspired by Galileo's studies on the pendulum, and would become important for Huygens's work on collision as well as that of the members of the Royal Society. This is absent in Descartes work.[189]

To defend his claim of a relationship between speed and "quantity of motion," which will also serve as a rationale for declining to investigate the measurement of speed, Descartes appealed in the letter to the idea that motion can be transferred between bodies, and his idea that a "quantity of motion" is "conserved" in all matter. The former idea had been articulated as his second law of nature in his work, *The World*, which had been written 6 to 10 years earlier (but would not be published until 1677).[190] The latter idea had also been articulated in this earlier work as that from which Descartes's first two

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[189] However, Descartes was familiar with some of Galileo's theoretical studies of the pendulum. For example, Descartes objected to Galileo's claim of isochrony in letters to Mersenne, arguing that air resistance and various environmental factors would interfere, likely making smaller oscillations slower than larger ones. AT I 73-4, 96. Descartes to Mersenne, 8 October, 13 November, and 18 December 1629. Beeckman had also been interested in the effect of air resistance on the pendulum (which he thought would affect small oscillations more than the large), and surmized that a pendulum is only isochronous in a vacuum. Beeckman, *Journal* 1:260. "If you can imagine this [oscillatory motion] as taking place in a vacuum, when only the tendency toward the center of the earth operates, it will perhaps correspond more exactly to what has been said [*i.e.*, perfect isochrony]; for the slowness of the motion [at the extremities of a large oscillation or in small oscillations] is greatly affected by the air with the result that the motion turns out to be even much slower." Quoted and translated in Ariotti, "Aspects of the Conception and Development of the Pendulum," 375. Descartes also claimed that Galileo's "demonstrations" of the circle as the brachistochrone were not sound -- they only suceeded in showing that a body descends more quickly along an arc of a circle than along the chord of the same arc. AT II 379-405. Descartes to Mersenne, 11 October 1638. Mersenne also disagreed with Galileo's claim of isochrony, but in the reverse of Descartes: smaller oscillations are faster than large ones. Later, Huygens, corresponding with Mersenne, also argued that the circular pendulum is not isochronous. Like Mersenne he contended that smaller oscillations are faster than large ones, but the periods of large and small oscillations can be made equal if the path of the pendulum is modified. He famously accomplished this by including "cheeks" to restrict the swing, which has the effect of gradually shortening the path of the pendulum bob. This shortened the period for larger amplitudes. Later Huygens, who led the way in the design of workable mechanical pendulum clocks, would show that a pendulum whose bob swept out the path of a cycloid rather than a circle is isochronous. See Piero E Ariotte, "Aspects of the Conception and Development of the Pendulum in the 17th Century," *Archive for History of Exact Sciences* 8 (1972): 329-410.

[190] AT XI 41. "I suppose as a second rule that, when one of these bodies pushes another, it cannot give the other any motion except by losing as much of its own at the same time; nor can it take away from the other body's motion unless its own is increased by as much." Translation by Mahoney, *The World.*

laws followed.[191] In the 1639 letter to de Beaune, both ideas are combined in a single sentence:

> there is a certain Quantity of Motion in all created Matter, which never increases nor decreases; and thus when a body causes another to move, it loses so much of its motion as it gives of it to it [the other body].[192]

This "Quantity of Motion" is not merely an amount of "speed." To clarify the point, he used a familiar example from experience—that of a stone falling from a high place and hitting the ground, about which he makes a surprising claim, particularly if one is familiar with his later works:

> ...if it does not return and is stopped, I conceive that that just shakes the earth, and thus transfers to it its motion; but if what it moves of the earth contains 1000 times more matter than it, transferring to it all its motion, it only gives to it a 1000th part of its speed.[193]

Before addressing what is surprising about this claim, let us first consider Descartes's reason for making it—which is to clarify the difference between "amount of speed" and "quantity of motion." The transfer of motion is not identical to the differences in speeds of each body before and after collision, instead one must take into consideration the size of the body in motion as well. It is notable that here as elsewhere Descartes does *not*

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[191] AT XI 43. After discussing his first two laws of nature, (the first being that a body will continue in the same state once it has begun moving "until collision with others forces it to change"), Descartes explains upon what these two rules follow: "Now it is the case that those two rules manifestly follow from this alone: that God is immutable and that, acting always in the same way, He always produces the same effect. For, supposing that He placed a certain quantity of motions in all matter in general at the first instant He created it, one must either avow that He always conserves as many of them there or not believe that He always acts in the same way." Translation by Mahoney, *The World*.

[192] AT II 543. Descartes to De Beaune, 30 April 1639. *Premierement, ie tiens qu'il y a vne certaine Quantité de Mouuement en toute la Matiere creée, qui n'augmente, ny ne diminuë iamais; et ainsi, que, lors qu'vn corps en fait mouuoir vn autre, il perd autant de son mouuement qu'il luy en donne.*

[193] AT II 543. Descartes to De Beaune, 30 April 1639. *[C]omme, lors qu'vne pierre tombe d'vn lieu haut contre terre, si elle ne retourne point, & qu'elle s'arreste, ie conçoy que cela vient de ce qu'elle ébranle cette terre, & ainsi luy transfere son mouuement; mais si ce qu'elle meut de terre contient mille fois plus de matiere qu'elle, en luy transferant tout son mouuement, elle ne luy donne que la milliesme partie de sa vitesse*. Note that if the body rebounds or returns, the quantity of motion is conserved. Quantity of motion here, as elsewhere in his writings, is scalar. This is a point of some importance, to which we will return later in the chapter.

define the quantity of motion as the product of the numerical measures of body and speed. The "Cartesian quantity of motion" has often been interpretted as an imperfect precursor to momentum, and has usually been expressed as the product of size and speed (akin to mass and velocity, *mv*). It has been considered "imperfect" because Descartes did not clearly specify what he meant by size, and because Descartes's quantity of motion is not a vector quantity as momentum is. Descartes could have easily expressed the quantity of motion as a product. He did not. The practice of doing so may well have contributed to some interpretive difficulties with Descartes rules of collision, as we will see more clearly in section 5. Whatever the quantity of motion is, Descartes held it to be true in principle that it remains the same. In *The World,* Descartes argued that this must be so because of the immutability of God; he used the same argument in *The Principles of Philosophy* (1644). In the 1639 letter to de Beaune, Descartes just maintained that it is so, without mentioning God. The resulting speeds, on the other hand, will depend on how much matter is in the bodies. To emphasize his point, Descartes wrote in the letter that if two unequal bodies have the same "quantity of motion," then the speed of the larger would be less than the smaller. A body with more matter has, "in a sense," more of what de Beaune seems to have called *Natural Inertia*:

> If two unequal bodies receive so much motion as each other, this similar quantity of motion does not give so much speed to the larger as to the smaller, one can say, in a sense, that the more a body contains of matter, the more it has of *Natural Inertia;* to which one can add that a body, which is large can better transfer its motion to other bodies, than a small one can, and that it can be moved less by them.[194]

[194] AT II 543. Descartes to De Beaune, 30 April 1639. *Et pour ce que, if deux cors inégaux reçoiuent autant de mouuement l'vn que l'autre, cette pareille quantité de mouuement ne donne pas tant de vitesse au plus grand qu'au plus petit, on peut dire, en ce sens, que plus vn cors contient de matiere, plus il a d'*Inertie Naturelle; *a quoy on peut adjouster qu'vn cors, qui est grand, peut mieux transferer son mouuement aux autres cors, qu'vn petit, & qu'il peut moins estre mû par eux.* The passage continues: "In this way there is one strength of *Inertia*, which depends on the quantity of matter, and another which depends on the extent

Large bodies can transfer their motion to other bodies better than small ones, and large bodies better resist being moved by other bodies. So, the amount of speed and the quantity of motion are not identical, and thus, the quantity of motion cannot be measured by the speed of a body alone. How fast a body moves is balanced by "how much matter it contains," or in the terminology of Descartes's friend de Beaune, its *Natural Inertia*. Descartes, however, did not think that de Beaune needed to appeal to a notion of *Natural Inertia* as something inherent to bodies. In a letter to Mersenne around the same time (December 1638), which also reveals that this is a topic that had been under discussion among de Beaune, Descartes, and Mersenne beyond what remains in the correspondence, Descartes argued against natural inertia as inherent to bodies, but attributed to de Beaune an example that illustrates what he meant by quantity of motion and its conservation:

> I do not consider that there is any natural Inertia or inherent slowness in bodies any more than does M. Mydorge. Further I believe that when a man walks, he causes the whole mass of the earth to move by however little it may be, but at the same time I agree with M. de Beaune that the greatest bodies being impelled by an equal force, as the greatest ships are by the same wind, always move more slowly than the others, which should perhaps be sufficient grounds for his reasons without calling in a natural Inertia which cannot be proved.[195]

Descartes closes the 1639 letter to de Beaune by underscoring the fact that he has never gotten around to determining anything concerning speed, but he maintained that the issues that distract him from considering speed have a great deal of value:

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of its area." *De façon qu'il y a vne forte d'*Inertie*, qui dépend de la quantité de la matiere, & vne qutre qui dépend de l'estenduë de ses superficies.*

[195] AT II 466-7. Descartes to Mersenne, December 1638. *Je ne reconnois aucune Inertie ou tardiueté naturelle dans les cors, non plus que M. Mydorge, et croye que, lors seulement qu'vn homme se promene, il fait tant soit peu mouuoir toute la masse de la terre, à cause qu'il en charge maintenant vn endroit, & aprés vn autre. Mais ie ne laisse pas d'accorder à M. de Beaune, que les plus grands cors, estant poussez par vne mesme force, comme les plus grands bateaux par vn mesme vent, se meuuent tousiours plus lentement que les autres; ce qui seroit peut-estre assez pour établir ses raisons, sans auoir recours à cette Inertie naturelle qui ne peut aucunement estre prouuèe.*

> You can see that there are many things to consider, before one can determine anything concerning Speed, and it is what always diverts me from it; but one can also account for many things by the method of these Principles, which one cannot attain formerly. As for the rest, I would not write to you so freely of these things, as I did not want to discuss this further, because the proof depends on my World, so I only hope that you interpret them favorably, and so I passionately wanted to testify to you what I follow.[196]

4.2 – Evidence of a fundamental change in Descartes's understanding of collision

Now let us consider what is surprising about Descartes's claim in the letter. The stone moves the earth! More generally, in two places in the letter Descartes is committed to the idea that a smaller body can move a larger body: once while illustrating the balance between size of body and speed in the quantity of motion ("a body which is large can better transfer its motion to other bodies, than a small one can, and that it can be moved less by them"[197]), and again explicitly while defending his notion that this quantity is conserved (a stone 1000 times smaller than the earth transfers 1000th of its speed to the earth; if it does not bounce, it "shakes the earth" a tiny bit[198]). Additionally, in the previous 1638 letter to Mersenne, regarding de Beaune, Descartes claims that "when a man walks, he causes the whole mass of the earth to move by however little it may be."

These statements are in conflict with Descartes's notorious (both to historians and Descartes's peers and immediate successors) "Fourth rule" of the *Principles of Philosophy*:

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[196] AT II 544. Descartes to De Beaune, 30 April 1639. *D'où vous pouuez voir qu'il y a beaucoup de choses à considerer, auant qu'on puisse rien determiner touchant la Vitesse, & c'est ce qui m'en a toûjours détourné; mais on peut aussi rendre raison de beaucoup de choses par le moyen de ces Principes, ausquelles on n'a pû cy-deuant atteindre. Au reste, ie ne vous écrirois pas si librement de ces choses, que ie n'ay point voulu dire ailleurs, à couse que la preuue en dépend de mon Monde, si ie n'esperois que vous les interpretez fauorablement, & si ie ne desirois passionnément vous témoigner que ie suis.*

[197] AT II 543. Descartes to De Beaune, 30 April 1639. *[V]n cors, qui est grand, peut mieux transferer son mouuement aux autres cors, qu'vn petit, & qu'il peut moins estre mû par eux.*

[198] AT II 543. Descartes to De Beaune, 30 April 1639. See note above.

> if the body C were entirely at rest, and if C were slightly larger than B; the latter could never move C, no matter how great the speed at which B might approach C. Rather, B would be driven back by C in the opposite direction: because a body which is at rest puts up more resistance to high speed than to low speed; and this resistance increases in proportion to the difference in the speeds. Consequently, there would always be more force in C to resist than in B to drive.[199]

One may be tempted to dismiss this rule as absurd with the evidence that one experiences smaller bodies moving larger bodies quite often in day-to-day life, they just need to move fast enough with respect to the larger body.[200] And besides, one might be tempted to imagine a case in which one body is only ever so slightly larger than the other—larger by a fleck of dust—and conclude that it seems quite odd that the ever so slightly smaller body will not be able to move the larger, no matter how fast the smaller moved. Descartes was aware of these lines of thinking. He fully acknowledged that several of his rules conflicted with day-to-day experience.[201] Descartes was not interested in bodies from day-to-day experience in the section of the *Principles of Philosophy* that contain his rules of collision, and his rules presuppose something that is actually impossible in his view of the world—two bodies separated from all others. Bodies that we seem to observe are surrounded and constantly in contact with other bodies, which affect their actions. There is no empty space. Moreover, he was not convinced that the best way to investigate the fundamental motions of matter was through experience. But it is important to note that

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[199] AT VIII 68. *Principia* II 49. Translation by Miller, *Principles,* 66.
[200] And, not considering friction, any speed would be enough to move the larger body.
[201] AT IX 93-4. *Principes* II 53. For example, just after Descartes presented the rules of collision, he wrote: "{Indeed, experience often seems to conflict with the rules I have just explained}. However, because there cannot be any bodies in the world which are thus separated from all others, and because we seldom encounter bodies which are perfectly solid; it is very difficult to perform the calculation to determine to what extent the movement of each body may be changed by collision with others. Since, {before we can judge whether these rules are observed or not}, we must simultaneously calculate the effects of all those bodies which surround the bodies in question and which affect their motion. These effects differ greatly, depending on whether the surrounding bodies are solid or fluid; and it is therefore necessary that we should immediately enquire into the difference between solid and fluid bodies." Translation by Miller, *Principles*, 69.

Descartes held these same views in his early writings as well as his mature *Principles*. Recall that according to his letter to de Beaune (1639), he never even got around to the measurement of speed, nor did he seem particularly interested in it.[202] The bodies that concerned him, and the rules by which they meet and interact, are not best accessed through observation, measurement, or experiment.[203] This is not to say that Descartes was completely disconnected from the empirically known world. Contrary to what Dijksterhuis has claimed, Descartes's physics is not reducible to mathematics in its objects and methods.[204] He relied on several principles from mechanics such as those we have encountered in the previous chapter on Descartes's older colleague Isaac Beeckman. Descartes also seems to have performed several experiments to investigate some of the principles underpinning his contest view of collision and the force of impact (percussion), which will be discussed below. He also performed some better-known experiments on the optical principles of the rainbow.[205] Nevertheless, Descartes seems to have held two very different views on one of the fundamental rules of collision—a rule that was a source of some controversy: what happens when a smaller body meets a larger body at rest.

[202] It is worth considering that in Huygens's "empirical" work on the topic of collision, he measures speeds indirectly using the pendulum. Huygens also shows through a variety of arguments (both mathematical and empirical, but primarily the former) that Descartes's rules of collision cannot be correct.
[203] In the letter to de Beaune, I have described one of the examples as being "from experience:" a stone falling to the earth. I have also emphasized that Descartes's rules appear to be about fundamental objects, which "experience" does not easily access (if it can at all). It is important to remember that the example from experience of a stone falling is to illustrate what Descartes means when he claims that there is a quantity of motion (distinct from speed) and this quantity never increases or decreases, but remains the same in all created matter. In other words it is an easy-to-imagine scenario that illustrates a fundamental principle.
[204] See appendix 1 and section 2 above.
[205] Spyros Sakellariadis, "Descartes' Experimental Proof of the Infinite Velocity of Light and Huygens' Rejoinder," *Archive for History of Exact Sciences* 26 (1982): 1-4. R. Hookyaas, "Beeckman, Isaac," in *DSB* 1: 567. Jed Buchwald, "Descartes' Experimental Journey Past the Prism and Through the Invisible World to the Rainbow," *Annals of Science* 65 (2007): 1-46. Also see Daniel Garber, "Descartes and Experiment in the Discourse and Essays," in *Essays on the Philosophy and Science of René Descartes*, ed. Stephen Voss (Oxford University Press, 1993), 288-310. Beeckman experimented on the motion of fluids, as well as the speed of light. Descartes responded to Beeckman's experiments on the speed of light. He also carried out his own famous experiments on the rainbow.

The view found in the letter is not exclusive to his comments to de Beaune. It can be found throughout his early writings. For example, in chapter 4 of Descartes's work, *The World*, which was likely written sometime between 1629 and 1633 (but left unpublished), he explicitly mentions the motion of bodies—particularly smaller bodies moving larger bodies—in his explanation of the action of a flame on wood. The action of a flame can "move" wood. This is because the flame is made of many small parts, which move very fast. Descartes writes: "I say also that their motion is very fast and very violent because [...] they would not have the force they have to act against other bodies if the quickness of their motion did not compensate for their lack of size."[206] Similarly, smaller bodies moving larger bodies at rest can be inferred from Descartes's account of his three elements (small, medium, and large bodies) in chapter 5 of *The World*, as well as his account of weight in chapter 11.

These examples, which seem to defy rule 4, are illustrations of other concepts and explanations of phenomena. Stones impacting the earth and shaking it, and a man walking causing the earth to move, illustrate the fundamental notion of the conservation of quantity of motion. The examples of fire and weight are phenomena that are explained by the more fundamental interaction of parts of matter acting according to the laws of motion. One might be tempted to think that rule 4 in the *Principles* is not in conflict with these prior claims in Descartes's earlier work because rule 4 is about fundamental

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[206] AT XI 8. "Now, insofar as it does not seem to me possible to conceive that one body could move another unless it itself were also moving, I conclude from this that the body of the flame that acts against the wood is composed of small parts, which move independently of one another with a very fast and very violent motion. Moving in this way, they push and move with them the parts of the body that they touch and that do not offer them too much resistance. I say that its parts move independently of one another because, even though several of them often act in accord and conspire together to bring about a single effect, we gee [sic] nonetheless that each of them acts on its own against the bodies they touch. *I say also that their motion is very fast and very violent because, being so small that we cannot distinguish them by sight, they would not have the force they have to act against other bodies if the quickness of their motion did not compensate for their lack of size*." Translation by Mahoney, *The World*, chapter 2, italics added.

collisions in the abstract. After all, Descartes himself admits in section 56 that "a solid body, immersed in a fluid, can be set in motion with very little force," since "the particles of fluids {tend to} move with equal force in all directions."[207]

However, on December 25 of 1639, seven months after Descartes's letter to de Beaune, Descartes explicitly mentioned the topic of collision in the abstract in a letter to Mersenne, which also conflicts with his "later view." Here he provides ratios to describe the effects of bodies of numerically different sizes and speeds that meet. He presented to Mersenne a quantitative description of two idealized scenarios in which two bodies collide, and a general inference. In the second scenario, a smaller body clearly moves a larger body at rest:

> [I]f a body of a certain size, which moves with a certain speed, meets another which is equal in size, and which does not have movement, it will communicate to it half of its own, so that they will both go together at half the speed that [it] did at first; but if it meets one which is double in size, it will communicate to it two thirds of its movement, and as they both together do not make more ground in three moments, than the first made in a moment. And generally, the more the bodies are large, the more they must go slowly, when they are pushed by the same force.[208]

The outcome of the scenario described in the second statement—a body meeting another at rest which is twice the size of the body in motion, and moving together after meeting—is clearly at odds with the result described in Descartes's 4th rule of collision in the *Principles.* In the latter, the smaller body will not move the larger.

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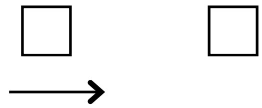
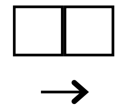
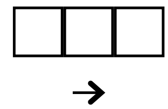
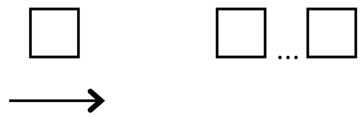
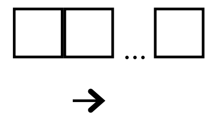
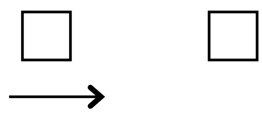
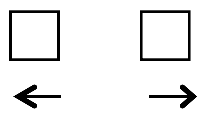
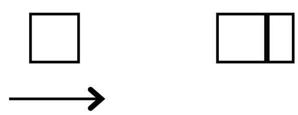
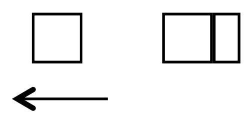
[207] AT IX 95. Translation by Miller, *Principles*, 70.

[208] AT II 627. Descartes to Mersenne, 25 December 1639. *[S]i vn corps de certaine grandeur, qui se meut de certaine vitesse, en rencontre vn autre qui luy soit esgal en grandeur, & qui n'ayt point de mouuement, il luy communiquera la moitié du sien, en forte qu'ilz iront tous deux ensemble de la moitié aussy viste que faisoit le 1er; mais, s'il en rencontre vn qui luy soit double en grandeur, il luy communiquera les deux tiers de son mouuement, & ainsy ils ne seront tous deux ensemble pas plus de chemin en trois momentz, que le 1er faisoit en vn moment. Et generalement, plus les corps sont grands, plus ilz doibuent aller lentement, lors qu'ilz sont poussez par vne mesme force*. According to the editor, the following is written in the margin of the manuscript copy of the letter: "This is contrary to his principles." *En marge de la Copie MS., on lit: 'Cela est contraire à ses principes.'*

Scenarios of collision from the 25 December 1639 letter to Mersenne		
	before bodies meet	*after bodies meet*
SCENARIO 1	 *s*	 *s*/2
SCENARIO 2	 *s*	 *s*/3
GENERAL INFERENCE	 *s*	 *s*/*n*

Scenarios of collisions with similar initial conditions to the above from the *Principles of Philosophy*		
	before bodies meet	*after bodies meet*
RULE 6	 *s*	
RULE 4	 *s*	 *s*

The scenario described in the first statement in the above quote—two bodies equal in size, one at rest, one moving to meet it—is the same initial conditions as that found in Descartes's 6th rule of collision in the *Principles of Philosophy*. What is described in the letter to Mersenne, on the other hand, is nearly identical to Isaac Beeckman's claims before Descartes's own, as well as John Wallis's views after Descartes, *i.e.* after they meet, the bodies move together with half the speed. In the *Principles of Philosophy,* Descartes produces a different outcome altogether: “[the body at rest] would be partly impelled by B and would partly repel it in the contrary direction [...]”[209] Significantly, what Descartes presents in the letter[210] is identical to what Isaac Beeckman had previously written.

That which is described in the letter is consistent with Descartes’s early view of collision. The conditions before the bodies meet in scenario 1 correspond to those in rule 6 in the *Principles*, and those in scenario 2 correspond to rule 4, although 4 is a general case in which the body at rest is larger by any amount. What happens to the bodies after they meet is inconsistent between the 1639 letter to Mersenne (representative of his early view) and the *Principles of Philosophy* (representative of his later view).

Whether Descartes was discussing collision in the abstract, or illustrating the conservation of quantity of motion with an example of the transfer of motion of a smaller body to a larger at rest, or explaining a phenomenon such as fire by the material contact of parts of matter, they all coincide. In every situation in his early view, smaller bodies transfer motion to larger bodies at rest. This is in opposition to rule 4 in Descartes’s later view.

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[209] AT VIII 69. "Sixth, if the body C were at rest and exactly equal in size to body B, which was moving toward it; necessarily, C would be to some extent driven forward by B and would to some extent drive B back in the opposite direction. Thus, if B were to approach C with four degrees of speed, it would {have to} communicate one degree to C, and be driven back in the opposite direction with the remaining three." Translation by Miller, *Principles,* 67-8.

[210] In each scenario one of the bodies is at rest, and the other moves toward it. In the first, the two bodies are the same size. In the second, the body at rest is twice as large as the first. In both, the quantity of motion is conserved. In the first, the body initially moves with some given speed. After the bodies meet, the body initially in motion pushes the body initially at rest. Since the bodies move together after the collision, the body is twice as large, and thus moves with half of the speed of the initially moving body. In the second scenario, a body again initially moves with some given speed toward the body twice its size at rest. After the bodies meet, the body initially in motion pushes the body initially at rest. Since the bodies are now moving together, the body is three times as large, and thus moves with a third of the speed. In other words the initially moving body communicated to the other two thirds of its movement. A general inference is made that “the more the bodies [at rest] are large, the more they must go slowly, when they are pushed by the same force” (AT II 627).

In none of these instances does Descartes describe the rebound of bodies. He touches on the topic in the letter to de Beaune with the conditional clause "if [the stone] does not return and is stopped, I conceive that that just shakes the earth, and thus transfers to it its motion" and he goes on to describe the miniscule amount of motion that would be transferred from the stone to the earth.[211] The structure of this statement is similar to the expressions of the impact law in *The World* and the *Principles*. Either a body will transfer its motion or it will rebound and not transfer motion. The outcome depends on whether or not the force of resistance is overcome. I have found no instances in Descartes early work, in which he describes a scenario where the force of resistance is not overcome. In other words, Descartes does not describe the conditions under which the force of resistance prevails, resulting in rebound. The focus is on the transfer of motion. Note that Descartes does not discuss the collision of fundamental parts of matter in our terms of elasticity or inelasticity. Similarly, the notion of perfectly hard bodies does not correspond to perfectly elastic collision. Some perfectly hard bodies interact with others, in Descartes account, in a way that appears to be what would later be called “inelastic”—the bodies move together after collision rather than rebound. Regardless, what is most important at this stage is the clear evidence of a fundamental change in Descartes’s understanding of collision: from initially arguing that a smaller body transfers motion to a larger body at rest, and later arguing that a smaller body does *not* transfer motion to a larger body at rest.

[211] See notes above and AT II 543.

4.3 – The impact law: overcoming the force of resistance to transfer motion

Descartes's "impact law" governs rebound and the transfer of motion between bodies, and operates according to the prior conservation of quantity of motion principle. In both *The World* and the *Principles*, the law states that when a body transfers motion to another, it gives the other as much as it itself loses, otherwise it rebounds retaining all of its motion.[212] The key to Descartes's "impact law" is the role of the *contest* between the "force of the moving body" and the "force of resistance" in the body being impacted. When the former wins the contest, motion is transferred. When the latter wins the contest, rebound occurs. In the *Principles*, both conditions of the law (transfer and rebound) are stated explicitly. And the conditions of the "contest view" of force,[213] which stipulate when transfer occurs and when rebound occurs in terms of a contest between forces, are clearly articulated. Both conditions in the "contest view" are expressed in Descartes's earlier work as well (such as *The World* and several letters between Descartes, Mersenne, and de Beaune), although not as precisely. Here Descartes placed his emphasis on the transfer of motion rather than rebound. According to Descartes's early work, it is possible that the force of resistance could "win the contest" against the force of the moving body, resulting in rebound, but Descartes did not specify the conditions in which the contest would be won in this manner. All of his early *quantitative* examples—even that of a stone hitting the earth, which is presumably a body with an immense force of resistance—describe the force of resistance being overcome resulting in the transfer of motion. Specifying the conditions in which the force of resistance is not overcome by the moving

[212] The "impact law" is stated as the second law in *Le Monde,* and the third law in *Principia.*
[213] Gabbey, "Force and Inertia," 243. Herival, *Background to Newton*, 49. The history of the "contest view" will be discussed in section 5.2.1 below.

force is precisely what Descartes later provides in the rules of collision in his *Principles of Philosophy*.

In *The World,* the emphasis of the "impact law" is on the transfer of motion, and it works together with the conservation of quantity of motion:

> I suppose as a second rule that, when one of these bodies pushes another, it cannot give the other any motion except by losing as much of its own at the same time; nor can it take away from the other body's motion unless its own is increased by as much [...][214]

In his explanation of the impact law in *The World,* Descartes describes how the transfer takes place in terms of resistance:

> [T]he motion of a body is not retarded by collision with another in proportion to how much the latter resists it, but only in proportion to how much the latter's resistance is surmounted, and to the extent that, in obeying the law, it receives into itself the force of motion that the former surrenders.[215]

Motion is *transferred* (and thus the motion of the initially moving body is retarded after the collision with the other body) in the following case: when the initially moving body *overcomes* the resistance of the body being impacted.

Although not explicitly stated in the second law itself, one can easily infer from subsequent discussions in Descartes's *The World* that the initially moving body *rebounds* if it does not overcome the resistance of the body being impacted; motion is not transferred and the initially moving body merely changes direction. This is discussed in the context of a comparison between particular qualitative examples from common experience, for instance air resistance retarding a moving stone (a lesser resistance retarding motion) versus a hard surface not slowing a moving stone (a greater resistance failing to retard motion):

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[214] AT XI 41. Translation by Mahoney, *The World*, chapter 7.
[215] Ibid.

> [...] if one thinks that the more a body can resist the more it is capable of stopping the motion of others (as one can, perhaps, be persuaded at first), one will, in turn, have a great deal of trouble in explaining why the motion of this stone is weakened more in colliding with a soft body of middling resistance than it is when it collides with a harder body that resists it more. Or also why, as soon as it has made a little effort against the latter, it at once turns on its heels rather than stopping or interrupting the motion it has.[216]

This passage illustrates the two scenarios that correspond to the two possible outcomes of the impact law: (1) in which the force of resistance is overcome and motion is transferred, and (2) in which the force of resistance is not overcome, no motion is transferred, and instead the moving body "at once turns on its heels rather than stopping or interrupting the motion it has." Note that here, as elsewhere, Descartes's concept of (quantity of) motion is scalar. He makes an explicit distinction between motion and direction. This was deliberate on Descartes's part, and was not merely a mistake in failing to use the notion of direction or, as we would say, a vector quantity in an algebraic equation.

The key to determine which outcome will occur is the amount of resistance a body offers to the colliding body. If the resistance is too great, and the colliding body cannot overcome it, then the body will lose none of its motion and will simply rebound (change direction). If on the other hand the resistance is not too great, and the colliding body can overcome it, then the body will transfer as much quantity of motion as it loses. If the colliding body "wins the contest" it transfers motion. If it "loses," it transfers no motion and changes direction. There is a discontinuity between the transfer of motion and rebound, which is determined by a body overcoming another's resistance.

The "contest view" of force is key to understanding Descartes's impact law not only in *The World* and the *Principles*, but it is essential for the proper understanding of Descartes's rules of collision as well. The rules will be discussed in section five. It is here

[216] Ibid.

that Descartes specifies the conditions in which the force of resistance is greater than the moving force. This is a key difference between his early and late account of collision. As we will see, although the contest view is apparent, and although Descartes clearly specified scenarios in which the force of resistance is greater, Descartes did not provide an especially clear explanation of what the *force of resistance* is. This has led to speculation among commentators and historians regarding the proper interpretation of the force of resistance in Descartes's rules of collision. Of particular interest has been the force of resistance in a body at rest. The traditional view has followed E. J. Aiton's interpretation, which took the "force of resistance" to be similar to an algebraic expression of the "quantity of motion." I will provide a revisionary interpretation of the force of resistance that corresponds to a reassessment of Descartes's rules of collision in section 5.3.4.

Section 5
Descartes's later account of collision

In the early 1640s, Descartes began working on a project, which in 1644 would be published as *Principia Philosophia*. It was amended and translated into French in 1647. This work is often regarded as Descartes's mature philosophy and is thought to be a development and synthesis of his earlier works such as *Le monde, Discours de la méthode,*[217] the essays appended to the latter,[218] as well as *Meditationes de prima*

[217] AT VI 1-78 *Discourse de la méthode pour bien conduire sa raison, et chercher la vérité dans les sciences* (1637)
[218] AT VI 81-227, 231-366, 369-485. *La Dioptrique*, *Les Météores*, and *La Géométrie*.

philosophia.[219] The Latin and French editions of *Principles of Philosophy*, together with some key correspondence, such as the famous letter to Clerselier in 1645 will serve as the source material for what I am calling “Descartes’s later view.”

As has been shown in section 4, there is a shift in how Descartes understood collision–particularly in the conditions of rebound and the transfer of motion. In his early view, a smaller moving body transfers motion to a larger body at rest. This is the case whether he is describing the transfer of motion in the abstract, illustrating another principle, or explaining a phenomenon such as fire. In his later view, codified in rule 4, a smaller moving body does not transfer motion to a larger body at rest. Only the determination is changed, and the initially moving body changes direction and continues to move with the same speed. Descartes’s change *to* the position described in rule 4 of the *Principia* was deliberate. It is not an oversight.

When we focus on the impact law, and the shift between Descartes's early and late account, we find an important connection between this law and the rules of collision. By focusing on the possible outcomes of the impact law, we find an underlying pattern running through the rules. I present this underlying pattern, and provide an alternative organization for the rules of collision, compared to the manner in which they are usually presented, which hides this pattern. The new organization reveals a common structure and method of Descartes's rules, and resolves several longstanding interpretive difficulties historians have faced regarding rule 4, the force of resistance of a body at rest, and the overall coherence of the rules of collision.

[219] AT VII 1-90. *Meditationes de prima philosophia, in qua Dei existentia et animæ immortalitas demonstratur* (1641)

5.1 – Overview of the rules of collision

In addition to a new order of the laws of nature, Descartes adds seven "rules of collision" to the *Principia*, which did not appear in *The World*. In keeping with his presentation beginning with the simplest principles to increasing complexity, the rules of collision present specific scenarios of two bodies of various sizes and speeds (or at rest), moving either with or toward each other. There are only two bodies involved in each rule and there are no oblique collisions. The bodies are taken to be perfectly hard and are taken to be separated from the otherwise present medium.[220] After his rules are presented and after he describes the distinction between a solid and fluid (where a fluid is entirely composed of parts of moving bodies), Descartes describes how bodies would interact in a medium.[221]

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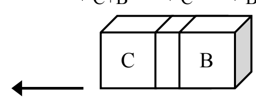
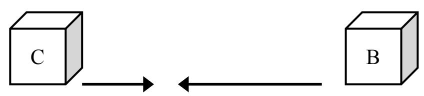
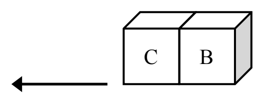
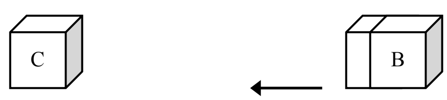
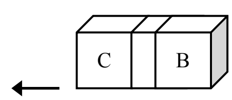
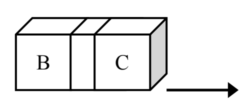
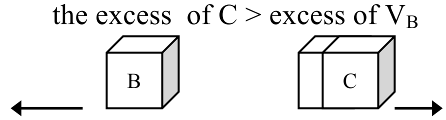
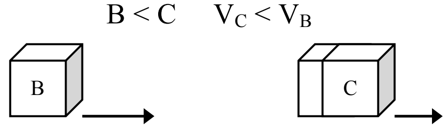
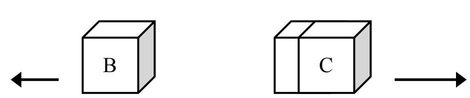
[220] AT VIII 67. *Principia* II 45. "...this could easily be calculated if only two bodies were to come in contact, and if they were perfectly solid, and separated from all others {both solid and fluid} in such a way that their movements would be neither impeded nor aided by any other surrounding bodies; for then they would observe the following rules." Translation by Miller, *Principles*, 64.

[221] AT VIII 71. *Principia* II 56. AT IX 95. *Principes* II 56. "That the particles of fluids {tend to} move with equal force in all directions; and that a solid body, immersed in a fluid, can be set in motion with very little force." Translation by Miller, *Principles*, 70. Also see AT VIII 70. *Principia* II 53. AT IX 93-4. *Principes* II 53.

Rules of Impact	
Before impact	After impact
1. $C = B$ $V_C = V_B$ 	
2. $C < B$ $V_C = V_B$ 	$V_{C+B} = V_C = V_B$ 
3. $C = B$ $V_C < V_B$ 	
4. $B < C$ C at rest 	V_B before = V_B after 
5. $C < B$ C at rest 	
6. $C = B$ C at rest 	
7a. $B < C$ $V_C < V_B$ 	If the excess of C < excess of V_B Then: 
7b. $B < C$ $V_C < V_B$ 	V_B before = V_B after V_C before = V_C after If the excess of C > excess of V_B Then: 
7c. $B < C$ $V_C < V_B$ 	If the excess of C = excess of V_B Then: 

Descartes did not present a table such as the above to accompany his rules. However, an image of cubes of differing sizes was included in the *Principles* to illustrate his rules of collision.[222]

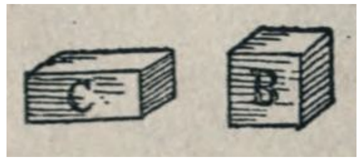


Figure 1. Principia, part II, section 56

Descartes also provided images of cubes of different sizes in his 1645 letter to Clerselier to specifically illustrate the fourth of the rules of collision.[223]

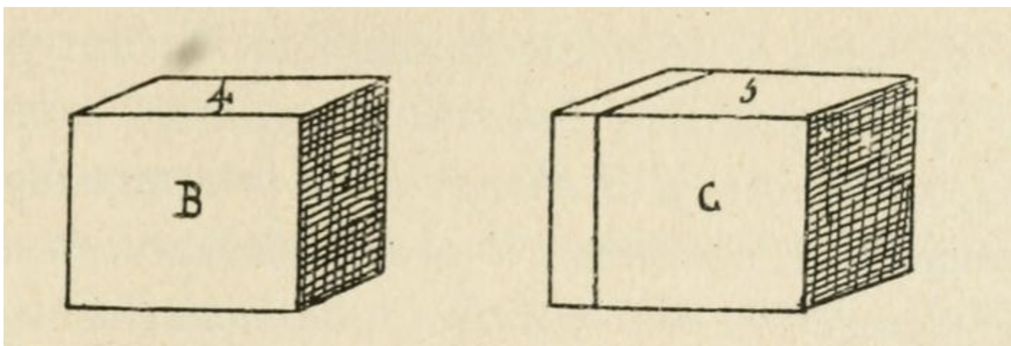


Figure 2. Descartes to Clerselier, 17 February 1645

Descartes provided little explanation of his rules in the Latin *Principia* (1644), writing: "These things require no proof, because they are obvious in themselves."[224] However, they were not obvious to his contemporaries, and Descartes was called on to explain himself. For example, he explained himself to his friend Claude Clerselier in a rather famous letter from 1645.[225] Clerselier was well versed in and sympathetic to Descartes's views. After Descartes's death, he would become his literary executor and was responsible for publishing three volumes of Descartes's correspondence (1657-

[222] AT VIII 68. *Principia* II 56.
[223] AT IV 185. 17 February 1645, Descartes to Clerselier.
[224] AT VIII 70. *Principia* II 52. Translation by Miller, *Principles*, 69.
[225] AT IV 183-7. Descartes to Clerselier, 17 February 1645. The letter has been translated and reproduced in Garber, *Descartes' Metaphysical Physics*, 260-2. Mahoney's translation is available on his website: http://www.princeton.edu/~hos/Mahoney/texts/descartes/desc-mot.html#Clerselier. Accessed January 25, 2015.

1667). Like many people, Clerselier seems to have been particularly confused by rule 4. Descartes provided some additional clarification in the French edition, *Les Principes de la Philosophie* (1647), even though Descartes himself claimed to Mersenne in April 1646 that he did not have more than “a quarter of an hour in the entire last year” to clarify his laws of motion.[226] Since the time of Clerselier, commentators have criticized and attempted to interpret Descartes's rules of collision, particularly the fourth rule.

5.1.1 – Interpretive difficulties: rules of collision and conceptions of motion

Descartes's rules of collision pose several interpretive problems. On the face of it, they seem to be inconsistent with the received view of Descartes's notion of (relative) motion.[227] The classic example, here, is the lack of symmetry between rules 4 and 5.[228]

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[226] AT IV 396. Descartes to Mersenne, 20 April 1646. “If you see M. Picot [the author of the French translation], please tell him that I have received his letters but that I cannot yet send him the continuation of his translation, because I have not yet succeeded, in the entire year which has passed since I reached that article, in finding a few moments in which to clarify my laws of movement.” Translation by Miller, *Principles*, 69n.

[227] Descartes's account of motion developed between *The World* and the *Principles of Philosophy*. In *The World* motion was simply "change of place." Descartes claimed that it was so simple that mathematicians assumed that motion is "easier to conceive of than the lines of mathematicians: the motion by which bodies pass from one place to another and successively occupy all the spaces in between." AT XI 40. Translation by Mahoney, *The World*, chapter 7. In the *Principles of Philosophy*, on the other hand, Descartes presents a two-fold definition, which includes a "vulgar conception" and a "proper conception." The former is similar to his previous account in *The World*. The latter defines motion as a transference of the neighborhood: “But if we consider what we should understand by motion not so much as it is commonly used but, rather, in accordance with the truth of the matter, then in order to attribute some determinate nature to it we can say that it is *the transference [translatio] of one part of matter or of one body from the neighborhood [vicinia] of those bodies that immediately touch it and are regarded as being at rest, and into the neighborhood of others.*” AT VIII 53-4. *Principia* II 25. Translation by Garber, *Descartes's Metaphysical Physics*, 159-60. The received view has interpreted this "proper conception" of motion to be relative motion. The passage cited to defend this view is the following: "[T]ransference is reciprocal; and we cannot conceive of the body AB being transported from the vicinity of the body CD without also understanding that the body CD is transported from the vicinity of the body AB, and that exactly the same force and action is required for the one transference as for the other." AT VIII 55-6. *Principia* II 29. Translation by Miller, *Principles*, 53. This has been called the doctrine of the "reciprocity of transfer."

[228] Rule 4 describes a small body approaching a larger body at rest. AT VIII 68. *Principia* (Latin) II 49. Rule 5 describes large body approaching a smaller body at rest. AT VIII 69. *Principia* (Latin) II 50. If one can arbitrarily choose a reference frame to describe motion, then the initial conditions of rules 4 and 5 are equivalent. However, the conditions described after collision in rules 4 and 5 are not equivalent. The

But, even with a more subtle understanding of Descartes's notion of motion (such as Garber's account), the rules remain problematic. With the latter approach, Descartes is understood to have made a meaningful distinction between motion and rest. Of primary importance to this difference is the immediate surrounding (*i.e*. neighborhood) of a part of matter. Motion is defined by the mutual separation (or transference) of the part and its neighborhood. Rest is defined by no transference between the part and its immediate surroundings.[229] In the rules of collision, however, the two bodies are described in

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smaller body rebounds after meeting the larger body at rest in rule 4, whereas motion is transferred from the larger moving body to the smaller at rest and both move together after collision in rule 5.

229 Garber has shown that Descartes did *not* deny the relativity of motion by arguing for something like "Newtonian absolute space" in which there is one privileged point of view that determines absolute motion and absolute rest -- regardless of whether one can perceive the difference, there is "a fact of the matter" with respect to absolute space. Rather, Descartes denied the relativity of motion in a very different way, but in a manner in which he could still argue that there is a non-arbitrary distinction between motion and rest -- that motion is not dependent upon a choice of point of view.

Motion as "change of place" (the "vulgar conception of motion") depends on an arbitrary choice of one place as being unmoved. Consider Descartes's example: "someone sitting in a boat while it is casting off from port thinks that he is moving if he looks back at the shore and considers it as motionless, but not if he looks at the boat itself, among whose parts he always retains the same situation." AT VIII 53. *Principia* II 24. Translation by Garber, *Descartes' Metaphysical Physics*, 162. Defining motion by "change of the neighborhood," on the other hand, is meant to limit this arbitrariness to only one choice: "Furthermore, I added that the transference take place *from the neighborhood of those bodies that immediately touch it into the neighborhood of others*, and not from one place into another since...what is taken as a given place varies [*loci acceptio varia est*] and depends upon our thought. But when we understand by motion that transference which there is from the neighborhood of contiguous bodies, since only one group of bodies can be contiguous to the mobile body at a given moment of time, we cannot attribute many motions to a given mobile body at a given time, but only one." AT VIII 55. *Principia* II 28. Translation by Garber, *Descartes' Metaphysical Physics*, 162-3. So, rather than choose any arbitrary place to determine what is and is not in motion, which can be done in innumerable ways, Descartes suggests that we consider only those bodies that directly surround another body (its neighborhood). There either will or will not be a transfer with respect to a body and those surrounding it. This does not depend on our thought.

However, given the doctrine of the "reciprocity of transfer," we cannot conceive of the difference between "a body transporting from its neighborhood" and "the neighborhood being transported from the body." In other words, although Descartes may have successfully limited the arbitrariness of motion to only one, it is still not clear if the body is at rest and the neighborhood is in motion or if the neighborhood is at rest and the body is in motion.

According to Garber, Descartes's response is that motion is the "mutual separation of two moving bodies." AT XI 656. Translation by Garber, *Descartes's Metaphysical Physics*, 167. Motion belongs to both the body and its neighborhood. It is "the mutual separation of a body and its contiguous neighborhood." The world is a plenum of moving bodies. No act of thought or choice of point of view will change whether or not a part of the plenum is in transference with respect to its contiguous neighborhood. No choice of point of view will make it seem as if there was transference when part of the plenum is not in transference with its neighborhood. Thus, Descartes maintains both the doctrine of reciprocity—which in this interpretation does not support relativity, but rather indicates that motion is the mutual separation of a body and its neighborhood—and that there is a clear distinction between motion and rest. When a body is at rest isolation. They are explicitly not surrounded by a neighborhood in the plenum. Thus, there is no meaningful way to describe a body at rest (according to Descartes's prior discussions), and yet three of the rules involve scenarios in which one body is at rest.

Costabel, Gabbey, and Garber have all argued that the rules of collision were added to the *Principles of Philosophy* late in the development of the work. Some have even claimed that they were hastily written and remain an unfinished draft.[230] As I will show below, the rules taken by themselves, are not as problematic as previous commentators have alleged. However, they do not appear to be in harmony with the entirety of Descartes's system. Specifically, the rules of collision are not constrained by Descartes's prior definitions of motion and rest.

5.2 – Impact law in the *Principles of Philosophy* (the contest model again)

In both the Latin and French *Principles,* the impact law remains largely the same as it was in *The World*. However, the various aspects of the "contest view" are more clearly stated in the law itself, rather than left to the commentary and explanation of the law:

> This is the third law of nature: when a moving body meets another, if it has less force to continue to move in a straight line than the other has to resist it, it is turned aside in another direction, retaining its quantity of motion and changing only the direction of that motion. If, however, it has more force; it moves the other body with it, and loses as much of its motion as it gives to that other.[231]

The law is expressed in two conditional parts, which correspond to the possible outcomes of a colliding body—rebound or transfer of motion. The two sections (41 and 42) that

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there is no transference with respect to the same contiguous neighborhood. See Garber, *Descartes' Metaphysical Physics*, 156-196.

[230] Garber, *Descartes' Metaphysical Physics*, 231, 242, 252. Gabbey, "Force and Inertia," 262-3.

[231] AT VIII 65. *Principia* II 40. Translation by Miller, *Principles*, 61.

follow the law are dedicated to “the proof of the first part of this law” and “the proof of the second part.” Rather than merely referring to whether resistance is overcome or not, the law in the *Principles* specifically refers to a “force of continuing” in the colliding body, which is in contest with a “force of resisting” in the body being collided into.

As was the case in the previous accounts of the impact law, there is a clear distinction between motion and direction, which is emphasized in the proof of the first part:

> the first part of this law is proved by the fact that there is a difference between motion considered in itself, and its determination in some direction; this difference makes it possible for the determination to be changed while the quantity of motion remains intact[232]

If the force of resistance is not overcome by the force of continuing, the colliding body changes its direction/determination but not its motion (this is due to the first law—things persist in their states).[233] And if the force of resistance is overcome by the force of continuing, the colliding body loses as much of its motion as it gives to the other. This is due to the conservation principle—“He conserves motion; not always contained in the

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[232] AT VIII 65. *Principia* II 41. Translation by Miller, *Principles,* 62.

[233] A year after the publication of the *Principia*, in the midst of an ongoing exchange of letters with Princess Elisabeth of Bohemia, the Princess wrote to Descartes on the topic of motion and direction, which underscores the importance of this particular distinction. After having “the leisure to read the philosophy of [Sir Kenelm] Digby, written in English” in which she hoped to find arguments that would challenge Descartes, she wrote to him instead to say: “I was completely astonished, when I arrived [at the relevant sections], to see that he has understood nothing – even less than what he approves in your account of reflection and of what he denies in your account of refraction. He does not make a distinction between the movement of a ball and its determination, and does not consider why a collision with a soft body reduces the speed, and that a hard body makes the other rebound!” *Je ne trouuay qu’vn peu deuant mon indisposition le loisir de lire la philosophie de M. le cheualier Digby, qu’il a fait en nglois, d’ou i’esperois prendre des argumens pour refuter la vostre, puisque le sommaire des chapitres me montroit deux endroits, ou il pretendoit l’auoir fait; mais ie fus toute estonnée, quand i’y arriuay, de voir qu’il n’auoit rien moins entendu que ce qu’il approuue de vostre sentiment de la reflexion, & de ce qu’il nie de celuy de la refraction, ne faisant nulle distinction entre le mouuement d’vne balle & sa determination, & ne considerant pourquoy vn corps mol qui cede retarde l’vn, & qu’vn corps dur ne fait que resister a l’autre.* AT IV 208-9. Princess Elisabeth to Descartes, 24 May 1645.

same parts of matter, but transferred from some parts to others depending on the ways in which they come in contact."[234]

Both Descartes's early and late views of collision operate according to the contest model of force and the conservation of quantity of motion. In Descartes's early view, although it is possible that the force of resistance is not overcome resulting in rebound, Descartes never stipulates a situation in which this takes place. Each quantitative example of collision that he provides involves the transfer of motion. In other words, the force of resistance is overcome and motion is transferred. In this, Descartes's early view of collision is similar to that of his colleague Isaac Beeckman. Descartes's later view, on the other hand, does stipulate situations when the force of resistance is not overcome, which results in rebound. This is the key difference between Descartes's early and late view. Rule 4 is central to this change. In his early view, smaller bodies move larger bodies at rest, *i.e.* they overcome the force of resistance, transfer motion, and move together with the formerly resting larger body after collision. In his later view, a smaller body cannot move a larger body at rest. The force of resistance is not overcome.

5.2.1 – Origins of the "contest view": Marcus Marci (1639) and the Scholastics

The "contest view" is found, as we have seen above, in Descartes's impact law in the *Principles* (1644)[235] and *The World* (1629-33),[236] as well as Descartes's discussions of

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[234] AT VIII 66. *Principia* II 42. Translation by Miller, *Principles*, 62.
[235] AT VIII 65. *Principia* II 40. "This is the third law of nature: when a moving body meets another, if it has less force to continue to move in a straight line than the other has to resist it, it is turned aside in another direction, retaining its quantity of motion and changing only the direction of that motion. If, however, it has more force; it moves the other body with it, and loses as much of its motion as it gives to that other." Translation by Miller, *Principles*, 61. Garber has indicated that "there is little in the way of significant addition in the French edition of Pr II 40-45, where Descartes sets out law 3, its explication and its defense. [...] There is reason to believe that Descartes hardly looked at the French translation of those sections. As Pierre Costabel has pointed out, there are some errors of translation in those sections that are

motion with de Beaune (1639).[237] Descartes appealed to the same contest view in other domains as well, such as optics and mechanical experiments. He relied on the "contest view" in his essay, *La Dioptriques*,[238] in which he explained the behavior of light using the analogy of tennis balls encountering hard and soft surfaces. Descartes also described a set of experiments with hammers, lead bullets, and French chefs, which operate according to the contest view in letters to Mersenne. The "contest view" was not unique to Descartes, and can be found in the work on collision of his contemporary Johannes Marcus Marci. The concerns that both faced, namely the conditions under which a force of resistance is overcome and motion is produced, have a long history, dating perhaps to the seventh book of Aristotle's *Physics*, as well as his *De caelo*, and more clearly in later scholastic commentaries.

In the *Principles* and *The World,* Descartes claims that a projectile should persist always in its motion, and yet projectiles actually slow down. This is due to air resistance. “For who is there who can deny that the air in which it is moving offers it some resistance? One hears it whistle when it divides the air.”[239] The projectile overcomes the resistance of the air, and some of its motion is transferred thus retarding its own motion.

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so glaring that Descartes could hardly have failed to notice them, had he but read them over with any care.” Garber, *Descartes' Metaphysical Physics*, 248.

[236] AT XI 41. *Le Monde*, chapter 7. “I suppose as a second rule that, when one of these bodies pushes another, it cannot give the other any motion except by losing as much of its own at the same time; nor can it take away from the other body’s motion unless its own is increased by as much. [...] [T]he motion of a body is not retarded by collision with another in proportion to how much the latter resists it, but only in proportion to how much the latter’s resistance is surmounted, and to the extent that, in obeying the law, it receives into itself the force of motion that the former surrenders.” Translation by Mahoney, *The World*.

[237] AT II 543. Descartes to De Beaune, 30 April 1639. Descartes’s explanation of motion relied on a combined expression of the conservation of motion and the law of impact. In his example of a stone hitting the Earth, Descartes specifically drew the distinction between rebound and transfer. *si elle ne retourne point, & qu’elle s’arreste, ie conçoy que cela vient de ce qu’elle ébranle cette terre, & ainsi luy transfere son mouuement*... “if the [stone] does not return and is stopped, I conceive that that just shakes the earth, and thus transfers to it its motion...”

[238] AT VI 96-100.

[239] AT XI 41. Translation by Mahoney, *The World*, chapter 7.

From common experience one knows that bodies are slowed by the air, or water, just as one knows that they are *not* slowed more when they hit something harder, which offers more resistance. In some cases they are not slowed at all, and only the direction of their motion changes. The relationships between resistance, transfer, and rebound are codified in the impact law contained in both *The World* and the *Principles.*

In *La Dioptriques,* Descartes used the analogy of a tennis ball in his account of the reflection and refraction of light. The reflection of light is a case in which a body meets another of greater resistance and changes its direction without losing any of its original speed. The refraction of light is described by the analogy of a tennis ball meeting another body of "middling resistance" such as a cloth tapestry. The analogous tennis ball meets a cloth whose resistance it overcomes and passes through, but in doing so its motion is retarded. Descartes used a similar example in his discussion with Mersenne in 1639 on the topic of the effect of light on white and black bodies respectively. He explained that the parts of black bodies deaden or absorb the action of subtle matter (the "pressure" of subtle matter being the cause of light), whereas "white bodies do not receive it in them, but throw it back." To illustrate this, he used the familiar following analogy: a black body is like "a tapestry which receives the movement of a ball that is pushed against it, and for this subject does not throw it back," but a white body is like "a hard wall, which is not at all shaken by this ball, [and] does not receive it; this is why it causes it to reflect."[240] The force of resistance is overcome in the case of a tapestry, and

[240] AT II 618. Descartes to Mersenne, 13 November 1639. *Pour les Cors noirs, vous sçauez que ie ne conçoy autre chose, par la Lumiere qui donne contre ces cors, que l'action, ou l'inclination à se mouuoir vers eux, qu'ont les parties de la Matiere subtile qui sont pousèes, par les cors qu'on nomme lumineux, vers ces cors qu'on nomme noirs. Or cette action peut estre amortie par les parties de ces cors noirs, à cause qu'elles la reçoiuent en elles-mesmes & ne la renuoyent point, au lieu que les parties des cors blancs ne la reçoiuent point en elles, mais la renuoyent: ainsi qu'vne tapisserie reçoit en soy le mouuement de la*

motion is transferred; whereas the force of resistance of a hard wall is not overcome, motion is not transferred, and the ball rebounds.

Stephen Gaukroger has closely linked Descartes's thoughts on the direction of motion (specifically "determination") to the tennis ball model regarding reflection.[241] He has also linked the tennis ball model from *La Dioptriques* to Descartes's impact law, claiming that "what Descartes has in mind here [*i.e.* the impact law] is the case of light rays being reflected from a surface, which he models on a tennis ball being reflected from a surface...[as] described in *La Dioptriques*."[242] In other words, I take Gaukroger to be claiming that the origin of Descartes's impact law (and implicitly the "contest view") comes from his thoughts on optics. This is not impossible. The focus of *The World* is on light, and the composition of the two works correspond: Descartes worked on *The World* between 1629-1633 and *La Dioptriques* was completed in draft between 1630-1632 before being published with the *Discours de la méthode* in 1637.[243] Moreover, this would not be the first time optics and collision would be linked: although Descartes probably was not familiar with the work, Thomas Harriot's manuscript on collision may well be connected to his investigation of the physical nature of light.[244] However, I have not encountered enough evidence to support the claim for the conceptual priority of Descartes's thoughts on reflection/refraction over his impact law, or vice versa. Rather, I think it is plausible that Descartes used a common pattern to explain the interaction of

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bale qu'on pousse contre elle, & pour ce sujet ne la renuoye point; mais vne muraille dure, qui n'est aucunement ébranlée par cette bale, ne le reçoit point; c'est pourquoy elle la fait reflechir.

[241] Stephen Gaukroger, *Descartes: An Intellectual Biography* (Oxford: Clarendon Press, 1995) 229-30.

[242] Gaukroger, *Descartes' System*, 122. Also see Gaukroger, *Descartes Biography*, 241-5.

[243] Gaukroger, *Descartes Biography*, xvi.

[244] See chapter 2.

bodies, whether in the form of the impact law, the reflection/refraction of light, or as we will see below, in a set of experiments with hammers and lead bullets.

Alan Gabbey has claimed in his highly influential article, “Force and inertia in the seventeenth century: Descartes and Newton,” that the "contest view" was fairly common in the 17th century, and that the concept was of "evidently anthropomorphic origin."[245] Although it does seem to have been fairly common, Gabbey, unfortunately, provides no evidence for the latter claim regarding its alleged anthropomorphic origin.

In an ongoing discussion from 25 December 1639 through at least 11 March 1640 with Mersenne, Descartes presented a set of experiments, which operate according to the basic contest view, just as his impact law and models for reflection/refraction. He seems to have offered the experiments as justifications of the concept to Mersenne, who appears to have been skeptical. In the midst of the conversation, Descartes wrote to Mersenne to explain the experiment:

> I am astonished that you have not yet agreed that one can better flatten a lead bullet with a hammer on a Cushion or on a suspended Anvil that can give way to the blow, than on a firm and immobile Anvil; for it is a very common experiment. And there are an infinity of similar ones in Mechanics which all depend on the same foundation: knowing it is not enough to flatten a lead bullet from a blow with so much force, but that it is necessary also that this force last some time, so that the parts of the bullet have leisure nonetheless to change location. Now when this bullet is on a firm anvil, the hammer jumps back up, almost at the same instant that it hit it, and thus does not have the time to flatten it, whereas if the anvil or other bodies which support this bullet, give way to the blow, the hammer remains longer pressed against the opposing thing.[246]

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[245] Gabbey, "Force and Inertia," 243.
[246] AT III 10. Descartes to Mersenne, 29 January 1640. *Ie m'étonne de ce que vous n'auiez pas encore ouy qu'on peut mieux applatir vne bale de plomb auec vn marteau, sur vn Coussin ou sur vne Enclume suspenduë & qui peut ceder au coup, que sur vne Enclume ferme & immobile; car c'est vne experience fort vulgaire. Et il y en a vne infinité de semblables, dans les Mechaniques, qui dépendent toute du mesme fondement: à sçauoir, ce n'est pas assez, pour platir vne bale de plomb, que de la fraper auec beaucoup de force, mais il fout aussi que cette force dure quelque temps, afin que les parties de cette bale ayent loisir cependant de changer de situation. Or quand cette bale est sur vne enclume forme, le marteau rejallit en haut, quasi au mesme instant qu'il l'a frapée, & ainsi n'a pas le loisir de l'aplatir tant que si l'enclume ou autre cors qui soutient cette bale, cedant au coup, fait que le marteau demeure plus long-temps appoyé de*

Although here Descartes puts the issue in terms of time, it is clear that the firm anvil offers too much resistance for the hammer to overcome and the hammer jumps back up instantly. Whereas, an anvil with less resistance (because it is supported by a cushion) allows the hammer to transfer some of its motion to the lead bullet, subsequently overcoming the resistance offered by the parts of the lead bullet which are generally at rest with respect to each other, and thus flattening it.[247] At least this seems to be Descartes's line of thinking.

Three months later Descartes continued to argue the point, and added some specifications to the experiment:

> In order to do this experiment well, it is necessary to use a hammer which is not very large: because if it had the force to entirely flatten the bullet on the anvil, it would not be able to do *more* on the cushion. And beyond that, one must put a plate of iron, or another body between the bullet and the cushion, so that it does not sink so much inside, being hit, that the hammer pressing against this cushion loses its force there.[248]

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contre. The topic had been raised in the previous month as well. See AT II 631. Descartes to Mersenne, 25 December 1639.

[247] It is notable that the experiments also correspond to at least one notion of the force of resistance that Descartes describes in both *The World* and the *Principles*. In his discussion of solids and liquids, he notes that solids offer more resistance than liquids because the parts of matter in solids are generally more at rest with respect to each other than the parts of matter in liquids, which are generally more in motion with respect to each other. He claims that there is no greater force/glue than this relative rest. In the experiments, Descartes describes the bullet being flattened as the parts of the bullet change location. In other words the "force of resistance," as the relative rest of the parts, is overcome and the parts move with respect to each other. See the following for Descartes's discussions of solids and fluids. AT VIII 71. *Principia* II 55. "That the parts of solid bodies are not joined by any other bond than their own rest {relative to each other}. Furthermore, our reason certainly cannot discover any bond which could join the particles of solid bodies more firmly together than does their own rest. For what could this bond be? It could not be a substance, because there is no reason why these particles, which are substances, should be joined by any substance other than themselves. Nor is it a mode different from rest; for no other mode can be more opposed to the movement which would separate these particles than is their own rest. Yet, besides substances and their modes, we know no other kinds of things." Translation by Miller, *Principles*, 70. Descartes had maintained such a position prior to the *Principles*, in *The World* as well. See AT XI 12-3. "Some force is necessary to separate them [the small parts touching one another but not moving away from one another], however small it may be. [...] Note also that twice as much force is necessary to separate two of them than to separate one of them, and a thousand times as much to separate a thousand of them. Thus, if it is necessary to separate several millions of them all at once, as is perhaps necessary in order to break a single hair, it is not surprising that a rather sensible force is necessary." Translation by Mahoney, *The World*, chapter 3.

[248] AT III 34. Descartes to Mersenne, 11 March 1640. *& pour bien faire cette experience, il faut se seruir d'vn marteau qui ne soit pas fort gros: car s'il auoit la force de platir entierement la bale sur l'enclume, il*

In the letter, Descartes included another "experiment," this one from the kitchens of Paris, to try to persuade Mersenne:

> But a more common experiment, which comes back to this same principle, and of which all the chefs in Paris will assure you, is that, when they want to break the bones of the shoulder of mutton with the back of a knife, they put it only on their hand or on a towel and hitting it, break it more easily than if it was on a table or on an anvil.[249]

These "experiments" appear to be in support of the contest view of collision. Motion is transferred when the force of the moving body overcomes the resistance of the other. An anvil placed on a cushion, or a shoulder of mutton placed on a towel (or in one's hand) reduces the resistance that the moving body must overcome. If it is reduced enough, the force of the moving body will overcome it and transfer motion to it. Once transferred, this quantity of motion overcomes the resistance of the bullet or shoulder of mutton itself, flattening the bullet or breaking the bones of the shoulder.

A contemporary of Descsartes, Johannes Marcus Marci, who was the personal physician to the emperors Ferdinand III and Leopold I, published *De proportione motus* in Prague in 1639.[250] The work contains what is likely the first published theory of collision.[251] According to Marci, the motion of a body is due to the continuous action of *impulsus*, which is a "transient quality." When the body is no longer directly connected to

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ne pouroit faire dauantage sur le cousin. Et outre cela, on doit mettre vne plaque de fer ou autre corps entre la bale & le coussin, afin qu'elle ne s'ensonse pas tellement dedans estant frapée, que le marteau appuyant contre ce coussin y perde sa force. The italics in the translation is not in the original.

[249] AT III 34. Descartes to Mersenne, 11 March 1640. *Mais vne experience plus vulgaire, qui reuient à ce mesm principe, & dont tous les cuisiniers de Paris vous assureront, c'est que, lorsqu'ils veulent rompre l'os d'vne eclanche de mouton auec le dos d'vn couteau, ils le mettent seulement sur leur main ou sur vne seruiette, & frapant dessus, le cassent ainsi plus aisement que s'il estoit sur vne table ou sur vne enclume.*

[250] E. J. Aiton, "Ioannes Marcus Marci (1595-1667)," *Annals of Science* 26 (1970): 153. Knud Erik Sørensen, "A Study of the *De proportione motus* by Marcus Marci de Kronland, Part 1" *Centarus* 20 (1976): 50.

[251] Knud Erik Sørensen, "A Study of the *De proportione motus* by Marcus Marci de Kronland, Part 2" *Centarus* 21 (1977): 257.

this action, the impulse diminishes.[252] In Marci's theory, the weight of a body serves as a resistance to motion.[253] On the event that two bodies collide, there is a *contest* between the impulse (of the moving body) and the resistance (*i.e.* the weight of the body being impacted). If there is an excess of impulse over resistance, then the motion of the body being impacted will be affected. If there is not an excess of impulse over resistance, the motion of the body will not be affected.[254] In Marci's theory the fourth porism is an example wherein the impulse of a smaller moving body does not exceed the resistance of a larger body at rest, and thus the smaller body rebounds without moving the larger body at rest. This is not only a clear manifestation of the "contest view," it is also remarkably similar to Descartes's fourth rule.[255]

Aiton has claimed that Marci's idea that "motion is determined by the excess of impulse over resistance, and indeed takes place only when there is such an excess"—which is at the heart of Marci's explanations of collision—is a "scholastic doctrine."[256] The relationship between motive force, motion, and the force of resistance is discussed by Aristotle himself in the *De caelo* and the seventh book of the *Physics*. And the specific topic of the impossibility of overcoming a larger force of resistance is discussed by later

[252] Aiton, "Marci," 155. See Johannes Marcus Marci, *De proportione motus seu, Regula sphygmica ad celeritatem et tarditatem pulsuum* (Prague, 1639), propositions 1, 2 and 9.
[253] Aiton, "Marci," 157.
[254] Sørensen, "Study, part 2," 262. " The impulse being designated as I, the supposition is that only if $I>m$ can the impulse set the body in motion, and the velocity is decided by $I = m \times v$." Also see Aiton, "Marci," 157. "[H]is explanations of collisions were based on the scholastic doctrine that motion is determined by the excess of impulse over resistance, and indeed takes place only when there is such an excess."
[255] Marci, *De proportione motus*, [95]. "*Porisma IV. Si globus minor percutiat majorem quiescentem, habeat verò majorem rationem ad suum impulsum, quam ad globum majorem, illo immoto reflectit minor.*" Aiton, "Marci," 157-8. "Scholastic ideas led Marci completely astray in Porism IV, where he envisaged a small ball striking a larger stationary ball, the small ball rebounding and the larger remaining at rest. In this case the impulse of the small ball was insufficient to overcome the resistance of the larger. The case in which the small ball had sufficient impulse to move the larger was covered in Porism III, where Marci stated that the small ball would either rebound or come to rest; in fact it always rebounds." Also see Sørensen, "Study, part 2," 263-5.
[256] Aiton, "Marci," 157.

commentators on Aristotle, such as John Philoponus, Thomas Bradwardine, and Nicole Oresme. According to Clagett, one can deduce from Aristotle's *Physics* (book VII) and *De caelo* that:

> Motion...requires some source of motion and a thing moved in continuous contact. More specifically it requires *force* and *resistance*. [...] In brief...with times and force constant, the distance traversed is inversely proportional to the resistance, or with distance and force constant, the time is directly proportional to the resistance--*with the proviso that the force is sufficient to cause movement.*[257]

In other words, "speed is proportional to the ratio of the motive force to the resistance, *provided that the force is sufficiently great to overcome resistance and produce movement.*"[258] The latter italicized proviso may be implied in Aristotle's *Physics* and *De caelo*. But later commentators appear to have been critical of Aristotle's simple ratio of motive force and resistance, for its apparent tension with the proviso.

John Philoponus rejected the simple force and resistance ratio, and opted for an arithmetical difference.[259] As Clagett has shown, Thomas Bradwardine was also critical of the ratio, but he "attempted to reconcile what he believed to be two facts: (1) No motion takes place when force is equal to, or less than, resistance (a fact of experience, also implied by Aristotle's discussion). (2) The factors of force and resistance as determiners of speed are in some kind of ratio rather than relatable by a simple arithmetical difference.[260] The worry regarding the simple ratio was that it seems to imply that any force, no matter how small, could move any resistance, no matter how large. Nicole Oresme stated this clearly in his commentary on the *De caelo* of Aristotle. After quoting *De caelo* and referring to the seventh book of the *Physics* Oresme states:

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[257] Clagett, *The Science of Mechanics in the Middle Ages*, 443-4, italics added. *Physics*, book VII, chapter 5 is quoted in Clagett, *The Science of Mechanics in the Middle Ages,* 428.
[258] Clagett, *The Science of Mechanics in the Middle Ages*, 432, italics added.
[259] Ibid., 437.
[260] Ibid., 437-8

> But, saving his reverence, it is not well stated, because from this statement it would follow that a power could move a resistance equal to itself and that any power, however small, could move any resistance, however large. ... This is illogical and impossible.[261]

It is conceivable that Marci's contest between impulse and resistance, and Descartes's contest between a moving body and the force of resistance, have a common source in the scholastic commentaries on the *Physics* and the *De caelo*. For Marci, motion is determined by the excess of the impulse over the resistance. For Descartes, the impact law, in both *The World* and the *Principles*, states that motion is transferred on the condition that the force of resistance is overcome, otherwise it is not. Both Marci and Descartes began their educations in Jesuit colleges.[262] However, establishing the exact connection to scholastic commentaries is an area for further research.

One of the leading characteristics of the change between Descartes's early account of collision and his later view is that in the latter he specifies the conditions wherein the force of resistance is greater and cannot be overcome. Although the impact law is stated in his early view as well (if the force of resistance is not overcome, no motion is transferred, resulting in rebound), Descartes does not specify the conditions wherein the force of resistance really is greater.

5.3 – Traditional organization of the rules of collision and interpretive problems

Several historians have noted that the rules of collision appear to be presented according to the initial conditions of the bodies in three general cases of bodies B and C

[261] Nicole Oresme, *On the Book of the Heavens and the World of Aristotle*, quoted in Clagett, *The Science of Mechanics in the Middle Ages*, 463-4.
[262] Aiton, "Marci," 153. Sørensen, "Study, part 1," 50. Gaukroger, *Descartes Biography,* 20-4, 38-61.

colliding.[263] Each case includes three specific scenarios. Case 1: B and C move toward each other in opposite directions. Case 2: One body (C) is at rest. Case 3: B and C move in the same direction with C preceding B, but with the speed of B larger than C, so that B will overtake C, resulting in a collision.

Case 1 B & C move in opposite directions	B = C, speed equal	rule 1
	B > C, speed equal	rule 2
	B = C, B moves faster than C	rule 3
Case 2 One body (C) at rest	B < C	rule 4
	B > C	rule 5
	B = C	rule 6
Case 3 B & C move in the same direction	C : B < speed B : speed C	rule 7a (Latin)
	C : B > speed B : speed C	rule 7b (Latin)
	C : B = speed B : speed C	rule 7c (French)

Under this organizational scheme, rules 2, 3 and 7a are interpreted as examples in which body B prevails in the contest and continues in its original direction of motion.[264] But, as we will see below, this organizational scheme obscures the structural similarities among the rules. The operation of rule 2, for example, is more closely related to the rules involving rebound (*e.g*. rules 4 and 7c) than rules 3 and 7a.

Those who have organized the rules in the manner above have also shared an assumption about the force of resistance and the force of motion. Descartes did not clearly express how the force of resistance should be quantified. In the absence of a clear statement, most have assumed that both the force of motion and the force of resistance

[263] Garber, *Descartes' Metaphysical Physics*, 238.
[264] Ibid., 239.

should be measured by the product of body and its speed. It is important to note that, although most modern interpreters have assumed that the forces of resistance and motion are to be measured by this product (typically expressed as *mv*), Descartes nowhere explicitly states this.[265] As we will see, Descartes did not state this because he did not intend for the force of resistance to be measured by the product; it has been an interpretive mistake to rely on this assumption.

With the traditional organizational scheme and the above-mentioned understanding of force, there have been several interpretational difficulties that commentators have perennially discussed. If the force of resistance is taken to be akin to the quantity of motion (interpreted as the product of body and speed *mv*), then the force of resistance in a body at rest would seem to be nil, since there is no speed. Some early modern thinkers held this very view, such as "Hobbes, the young Leibniz, Malebranche,"[266] as well as William Neile, who was a member of the Royal Society and a correspondent with John Wallis on the topics of motion and collision in the 1660s.[267] But clearly, Descartes did not hold such a view. It runs counter to the outcomes described in Descartes's rules, particularly rule 4.

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265 Ibid. "B seems to win the contest in two circumstances, when it has the same speed as C but is larger than C (R2), and when it is the same size as C but is moving faster (R3). ... This suggests that in moving bodies, both the force for proceeding and the force of resisting are functions of size and speed alone, and are jointly proportional to them. It is important to recognize that Descartes does not say this explicitly, but it isn't too much to infer that the forces in question are measured by the product 'size times speed'. This seems to be what he has in mind in R7 as well..." Also see, for example, John Roche, *Mathematics of Measurement: A Critical History* (London: Athlone Press, 1998) 106. "In calculations on collisions Descartes makes the sum of the products of notionally assigned integers representing quantity of matter and velocity respectively, of two colliding bodies equal before and after a collision. It is clear, therefore, that he thought of quantity of motion as proportional to the product of the numbers representing quantity of matter (or weight) and velocity. However, I have been unable to find a passage in which he states this formally, nor does he use algebra in this context. This is not surprising since Descartes is not presenting an argument in a rigorous mathematical manner here, preferring a more descriptive approach."
266 Gabbey, "Force and Inertia," 244.
267 See chapter 5.

The standard response to this difficulty has followed E. J. Aiton, who maintained that the force of resistance is similar to the algebraic expression of the "quantity of motion" (*i.e.* the product of body and its speed). And the force of resistance in a body *at rest* is measured by the product of the body and the speed of the body *approaching*. This view of the force of resistance in a body at rest has been widely accepted. It has also informed several other important theses, such as Gabbey's argument that Descartes's notion of force is an intermediary between the traditional "contest view" and a "Newtonian view" of force.[268] It is also key to Garber's presentation of the development of Descartes's rules of collision from the Latin edition of the *Principles*, to Descartes's explanations in the letter to Clerselier, to the French edition of the *Principles.* However, such a view of the force of resistance relies on an unwarranted expression of force (the product *mv*) that historians such as Aiton likely developed through an overreliance on an algebraic expression of Descartes's ideas that Descartes himself never used. The received view of the force of resistance in a body at rest will be investigated and criticized more thoroughly below in section 5.6.

An additional problem noted by previous historians, such as Aiton, Gabbey, and Garber, is the relationship between the impact law and the rules involving equal forces. According to these historians, rules 1, 6, and 7c (all of which involve an equal balance of forces, if the forces are understood as the above mentioned products) are not necessary

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[268] Gabbey, "Force and Inertia," 244-5. Descartes maintained, contra Hobbes et al. that “a body at rest reacts against any attempt to set it in motion, thus giving rise to a contest between opposing forces. However, it was Newton who fully realised that the opposing forces must be reinterpreted (as *vis impressa* and *vis inertiae*) so that they are always and necessarily *equal*, and consequently that each force does not depend on the (whole) initial speed of the striking body, but only on the difference between it and the final speed. ... In other words, Newton saw that the opposing forces actually involved in an interaction do not *determine* the exchanges of motion, as is the case according to the traditional view, but are the dynamical *expression* of these exchanges. As I shall show, in this respect Descartes is partly (though mainly) traditional, and partly Newtonian: he occupies an especially peculiar position in this revolutionary developmentin the functional role ascribed to force in the seventeenth century.”

consequences of the general impact law, a law which relies on a contest between forces.[269] Aiton attempted to resolve this by claiming that 7c may be an interpolation between 7a and 7b, just as 1 is a limiting case between 2 and 3. Others, such as Gabbey and Garber, have suggested that Descartes must be relying on an implicit symmetry principle.[270]

5.4 – Underlying organization: outcomes of the impact law

A different picture of Descartes's theory of collision emerges when we focus on the structural similarities of the rules, rather than the initial conditions. Doing so, we uncover a pattern that is indicative of the likely manner that Descartes originally thought of the problem of collision, before he organized the rules into the presentation found in the *Principles of Philosophy.* Seeing the rules in this new light also resolves several of the interpretive difficulties related to the traditional organization.

The new order is congruent with the main features of Descartes's impact law—whether the force of resistance is overcome resulting in a transfer of motion, or if it is not overcome resulting in rebound. In Descartes's introduction (section 45) to the rules of collision (sections 46-52), he himself places emphasis on the contesting forces involved in his impact law:

> In order to determine, from the preceding laws, how individual bodies increase or decrease their movements [*i.e.* the effects of a transfer of motion] or turn aside in different directions [*i.e.* rebound] because of encounters with other bodies; it is only necessary to calculate how much force to move or to resist movement there

[269] Aiton, "Vortex Theory," 252. Garber, *Descartes' metaphysical physics*, 241, 5. Gabbey, "Force and Inertia," 264. Aiton has argued that rules 1 and 7c are not necessary consequences of the general law.
[270] Gabbey and Garber discuss this specifically regarding rule 6 in the context of the so-called principle of least modal mutation (PLMM) or change (PLMC) as found in the 1645 letter to Clerselier. This letter will be discussed below.

> is in each body; and to accept as a certainty that the one which is the stronger will always produce its effect.[271]

To determine whether a transfer of motion or rebound occurs, one only needs to calculate the moving force and the force of resistance. The rules can thus be organized according to the outcomes: rebound, transfer, or both (in the case of equal forces). It is notable that Descartes maintained this emphasis while introducing the rules in both the Latin edition (1644) as well as the French edition (1647). Moreover, when he was called upon by his friend Clerselier in 1645 to further explain himself, particularly the rules wherein one body is at rest, Descartes began by citing the impact law: "it is a law of nature that a body that moves another must have more force to move it than the other has to resist it." Then, later in the letter, he reiterated the outcomes—rebound, transfer, or both—which correspond to the sizes of the forces:

> {W}e must notice that they {body B in motion and body C at rest} can become compatible in two ways, namely, *if B changes the entire determination of its motion* {*i.e.* rebound}, or, *if it changes body C's rest, transferring to it such part of its motion as will enable it [B] to push C in front of it as fast as it will itself go* {*i.e.* transfer of motion}. And I have said nothing in these three rules {4, 5, and 6} but this, that when C is larger than B, it is the first of these two ways which takes place, and when it is smaller, it is the second, and finally, when they are equal, this change is made half in the one way and half in the other.[272]

We should take Descartes by his own words and consider his rules according to the outcomes of collision (*i.e*. rebound, transfer of motion, or both), as well as the conditions of the impact law (*i.e*. the contest between the force of moving and the force of resistance). Organizing the rules according to outcome provides insight into Descartes's understanding of the forces involved in collision.

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[271] AT VIII 67. *Principia* II 45. Translation by Miller, *Principles*, 64. Additions in brackets included by me.
[272] AT IV 186. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261-2. Square brackets are in Garber's translation. Curly brackets indicate clarifying additions by me. Italics from the AT.

In the new organization, the first set of rules includes those scenarios (rules 4, 2, and 7b) in which the force of resistance is not overcome, resulting in rebound. The second set of rules (5, 3, 7a) includes those in which the force of resistance is overcome and motion is transferred between the bodies. The third set includes scenarios in which there are degrees of symmetry, resulting in both rebound and transfer, as is the case in rules 6, 7c, and 1.

5.4.1 – Set 1: Rebound, the force of resistance is not overcome – rules 4, 2, 7b

Rule 4 clearly stipulates a condition in which the force of resistance is not overcome by the moving force:

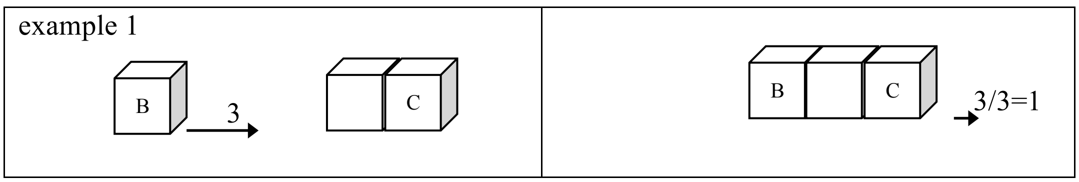
> The fourth [rule] is that if body C were just a bit larger than B, and were entirely at rest, *that is, not only is there no apparent motion, but also it is not surrounded by air nor by any other fluid bodies, which as I shall discuss below [in Pr II 59] dispose the hard bodies they surround to be more easily moved,* then whatever the speed with which B could go toward it [C], *it would never have the force to move it,* but it would be forced to rebound in the same direction from which it had come, since *seeing that B could not push C without making it go as fast as it itself would go afterwards*, it is certain that C should resist more the faster B goes toward it, and *its resistance ought to prevail over the action of B because it is larger than it.*[273]

As has been discussed in section 4 above, in Descartes's early view smaller bodies in motion overcome the force of resistance of a larger body at rest, transferring motion to the larger bodies at rest. After the impact, both move together. In rule 4 Descartes explains, using several examples, that *if* motion *were to be* transferred when a smaller body meets a larger at rest, it *would* transfer motion in a particular manner. This is precisely the manner described in his early view, and that which was described by

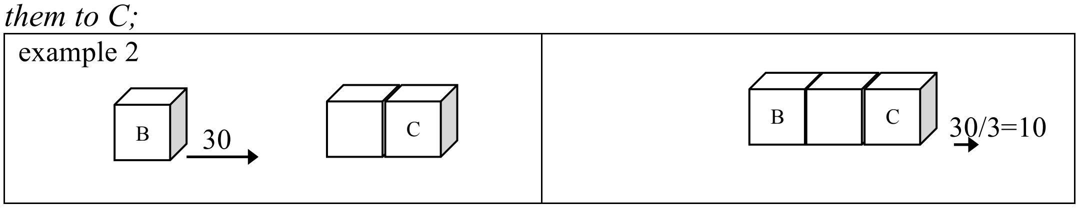
[273] AT IX 90-1. *Principes* (French) II 49. Translation by Garber, *Descartes' Metaphysical Physics*, 257. Italics in Garber's translation indicate additions to the rule in the French edition, which are absent in the Latin edition.

Beeckman before him. The numerical examples Descartes uses in rule 4 to illustrate this possibility are identical in form to the examples he used in his letter to Mersenne in 1639. The following are from rule 4:

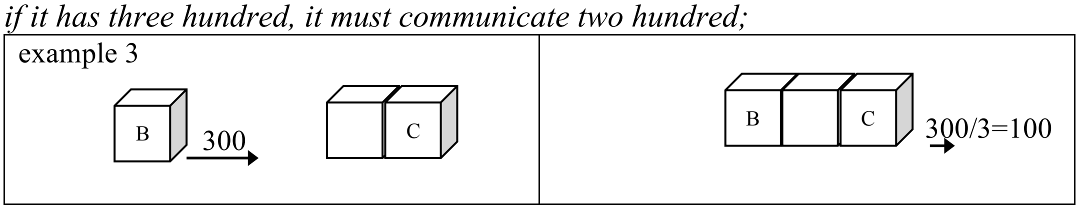
> *Thus for example, if C is twice as large as B, and B has three degrees of motion, it cannot push C, which is at rest, without transferring to it two degrees (namely, one for each of its halves) and without retaining only the third for itself, because it is not greater than each of the halves of C and afterwards, it cannot go faster than they go.*



> *In the same way, if B has thirty degrees of speed, it must communicate twenty of them to C;*



> *if it has three hundred, it must communicate two hundred;*



> *and so [it] always [communicates]twice what it retains for itself.*[274]

In the third example, for B to push C, B would have to transfer 200 degrees of motion. In other words, the initial speed of B was 300, and after they meet, the quantity of motion is conserved, so they move together at a speed of 100. Since 300 – 100 = 200, two hundred degrees of speed was transferred. This is precisely what would have had to be transferred according to Descartes's early view.

[274] AT IX 90-1. *Principes* (French) II 49. Translation by Garber, *Descartes' Metaphysical Physics*, 257-8.

But motion is *not* actually transferred in the scenario in rule 4, because Descartes has stipulated this as a condition in which the force of resistance is not overcome:

> *But since C is at rest, it resists the reception of twenty degrees ten times more than it resists the reception of two, and it resists the reception of two hundred one hundred times more, so that the more speed B has, the more resistance it finds in C. And since each half of C has as much force to remain in its rest as B has to push it, and since both resist at the same time, it is evident that they ought to succeed in forcing it [i.e., B] to rebound.* Consequently, whatever the speed with which B goes toward C, at rest and larger than it, it will never have the force to move it.[275]

This condition is merely *size*. Contrary to the received view, the force of resistance in a body at rest is not the product of the size and the speed of the body approaching.

Rule 2 is structurally similar to rule 4 in both the outcome of the collision, and the manner in which that outcome is produced. In rule 2, body B is larger than body C.[276] The speed of B and the speed of C are equal (although in opposite directions). As is the case in rule 4, when the smaller body C meets the larger body B, *no motion is transferred.* Body C *rebounds* retaining all of its motion. As in rule 4, the larger body is unchanged. Since both bodies have equal speeds, and since the smaller body now moves in the same direction as the larger, after the collision they move together, each retaining their original speeds.[277]

In the French edition of the *Principles,* Descartes divides rule 7 into three parts. Rule 7b is again structurally similar to rules 4 and 2. In the scenario described in this rule,

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[275] AT IX 90-1. *Principes* (French) II 49. Translation by Garber, *Descartes' Metaphysical Physics*, 258.

[276] AT IX 90. *Principes* (French) II 47. "The second [rule] is that, if B were the slightest bit larger than C, and they encountered one another with the same speed, then only C would be reflected in the direction from which it came, and afterward both would continue their motion together in the same direction. *For B, having more force than C, could not be forced to rebound by it*." Translation by Garber, *Descartes' Metaphysical Physics*, 256.

[277] Compare this to Garber's account of the second rule. Although Descartes does *not* mention any transfer of motion, and although Descartes does in fact describe the smaller body reflecting after meeting the larger, Garber interprets this rule as similar to rules 3 and 7a. Perhaps this is because both bodies move together after impact. However, this is only because they initially had the same speed and both retain their speeds after impact because no motion had been transferred. See Garber, *Descartes' Metaphysical Physics*, 238-9.

bodies B and C move in the same direction. Body C precedes body B, but the speed of B is greater than C such that B will overtake C. If C is larger than B, and if the amount that C is larger than B is greater than the amount that the speed of B is greater than the speed of C, then B will rebound and retain all of its motion.[278] The force of resistance is not overcome and no motion is transferred.

In each case (4, 2, and 7b), a transfer of motion *would* take place, *if not for* the force of resistance prevailing in the contest. The key condition in each of these scenarios is *size*. Because the resting body is larger in rule 4, no motion is transferred and the smaller rebounds. Because body B is larger in rule 2, body C rebounds and no motion is transferred. Body B remains in its original state and continues to move with its original speed (and body C, having the same speed, moves together with it). In rule 7b, body C is not only larger than body B (in which B moves in the same direction as C and will eventually catch up to it), the amount C is larger than B is greater than the amount the speed of B is larger than speed of C. In this scenario, no motion is transferred and body B rebounds retaining all of its motion.

Rule 4 is the key scenario. It stipulates precisely the condition when the force of resistance is greater than the moving force, which results in no transfer of motion but rather rebound. Rules 2 and 7b are variations of rule 4, with added degrees of complexity. Rule 2 is identical to rule 4, except that the larger body is now also in motion, and the

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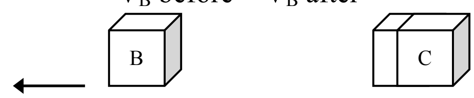
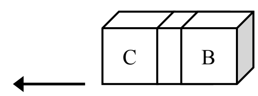
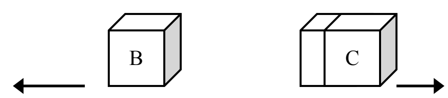
[278] AT IX 92-3. *Principes* (French) II 52. "The seventh and last rule is that if B and C go in the same direction, and C precedes but goes more slowly than B, so that at length, [B] would hit it [C], then it could happen that B would transfer a part of its speed to C in order to push it [C] in front of it [B], and it could also happen that it could transfer none at all, but rebound with all of its motion in the direction from which it came. Indeed, [these outcomes can happen] *not only when C is smaller than B*, but also when it is larger; [...] when that with which the size of C surpasses that of B is greater than that with which the speed of B surpasses that of C, B must rebound, without communicating any of its motion to C. [...] This can be calculated as follows. If C were exactly twice as large as B, and B did not move twice as fast as C, but lacked something of it, then B should rebound without increasing the speed of C..." Translation by Garber, *Descartes' Metaphysical Physics*, 260.

speeds of both bodies are equal. In rule 7b, not only are both bodies in motion, they move in the same direction, and the speeds are not identical.

Rebound Force of resistance is not overcome No transfer of motion	
Before impact	After impact
4. $B < C$ C at rest 	V_B before = V_B after 
2. $C < B$ $V_C = V_B$ 	$V_{C+B} = V_C = V_B$ 
7b. $B < C$ $V_C < V_B$ 	V_B before = V_B after V_C before = V_C after If the excess of C > excess of V_B Then: 

5.4.2 – Set 2: Transfer, the force of resistance is overcome – rules 5, 3, 7a

As previously mentioned, all of Descartes's examples of collision in his early view involved the transfer of motion with no rebound. In his later view, in the fourth rule in the French edition of the *Principles*, as we just saw, he provides several quantitative examples to illustrate his line of thinking. Three of them describe the amount of motion that *would* be transferred when a small body collides with a larger at rest, *if* the small body could overcome the force of resistance. A straightforward transfer of motion, of the kind that we see in Descartes's early view, as well as Beeckman's writings on collision, is what *would* take place. It does not, because he has chosen the scenario in rule 4 to stipulate the condition when the force of resistance cannot be overcome. Rule 5, on the other hand, *is* a straightforward transfer of motion. The body at rest, C, is the smaller body. As such, its force of resistance can be overcome by any larger moving body, B. When the larger moving body, B, meets the smaller body at rest, C, the larger pushes the smaller. Specifically it transfers however much of "its motion [is] necessary to bring it about that they would afterwards go at the same speed."[279]

Descartes provides two numerical examples, which he describes extensively to illustrate this rule. His reasoning does not rely on a symbolic expression of the quantity of motion as the product *mv* (in which case the quantity of motion of the body at rest would be zero), nor does he use symbolic algebra to derive the outcome. Rather, his reasoning is strikingly similar to that of Beeckman, which we have encountered in chapter 2:

> Namely, if B were twice as large as C, it would only transfer to it a third of its motion, since that one third would move C as fast as the two remaining ones would move B, since it is assumed to be twice as large. And so, after B encountered C, it would go slower by one third than it went before, that is, in the time that it could have traversed three spaces, it could only traverse two. In the same way, if B were three times larger than C, it would only transfer to it a fourth of its motion, and so on.

The speeds before and after collision are balanced by the size of the moving body before and after collision. In other words, there is an inverse proportion of the size of the initially moving body to the larger size of the body after the collision (since they move together), and the speed of the initially moving body to the speed of the moving body after collision.

Rule 3 is similar to rule 5 in that the outcome of the *transfer* of motion is such that the two bodies move together after collision. Similar to the relationship of increased complexity between rules 4 and 2 in the previously described set, rule 3 introduces added

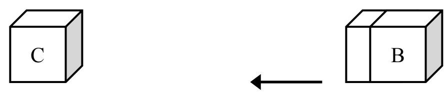
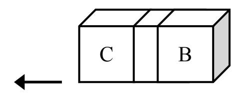
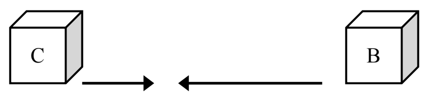
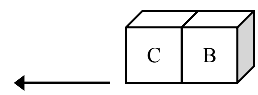
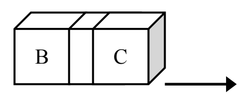
[279] PR II 50. Translation by Garber, *Descartes' metaphysical physics*, 258.

complexity to rule 5. Just as one body is at rest in rule 4, whereas both are in motion in rule 2, so too is one body at rest in rule 5 and both are in motion in rule 3. In addition, Descartes describes the operation of the collision in rule 3 in two parts. Although the ultimate outcome of the collision is the transfer of motion, the first part of the rule involves rebound. Bodies B and C are equal in size. B and C move in opposite directions toward each other, and the speed of B is larger than the speed of C. In the first part of rule 3, body C does not overcome the force of resistance of body B, which moves faster than C. Thus, C rebounds. It would retain its motion, and no motion would be transferred. However, since the speed of C is less than the speed B, and after rebounding C precedes B, body B will push body C. This is the second part. Body B moving faster than body C (and in the same direction) will transfer however much motion is required so that they move together.[280]

The second part of 3 is similar to rule 7a. They share the following characteristics. Both bodies move in the same direction, body C preceding body B. When body B overtakes C, it pushes body C. In other words, body B transfers enough motion that they move together after the collision. In the second part of rule 3, bodies B and C are the same size. In 7a, body C is larger than body B. "As long as that with which the size of C

[280] AT IX 90. *Principes* (French) II 48. "The third [rule] is that if these two bodies were of the same size, but B had the slihgtest bit more speed than C, then not only would it happen that after the encounter, C alone would rebound and both would move off together, as before [i.e., in Rule 2], but also it would be necessary for B to transfer to C half of the speed by which the one exceeded the other, *because [C] being in front of it, it could not go any faster than it [C].* That is, if before their encounter, B had had, for example, six degrees of speed and C had had only four, *it [B] would transfer one of its excess degrees of speed to it [C],* and thus afterwards both would go *[iroient]* with five degrees of speed, *since it is easier for B to communicate one of its degrees of speed to C than it is for C to change the entire course [cours]* of the motion B has." Translation by Garber, *Descartes' metaphysical physics*, 256-7.

surpasses that of B is less than that with which the speed of B surpasses that of C, B should never rebound, but push C, transferring into it one part of its speed."[281]

Transfer of motion Force of resistance is overcome	
Before impact	After impact
5. C < B C at rest 	
3. C = B $V_C < V_B$ 	
7a. B < C $V_C < V_B$ 	If the excess of C < excess of V_B Then: 

5.4.3 – Set 3: Degrees of symmetry – 6, 7c, 1

Rules 6, 7c, and 1 are unique. The previous sets of rules have described scenarios in which the force of resistance is not overcome and no motion is transferred (set 1), or the force of resistance is overcome and motion is transferred (set 2). In the scenarios

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[281] AT IX 92-3. *Principes* (French) II 52. "The seventh and last rule is that if B and C go in the same direction, and C precedes but goes more slowly than B, so that at length, [B] would hit it [C], then it could happen that B would transfer a part of its speed to C in order to push it [C] in front of it [B], and it could also happen that it could transfer none at all, but rebound with all of its motion in the direction from which it came. Indeed, [these outcomes can happen] *not only when C is smaller than B*, but also when it is larger; as long as that with which the size of C surpasses that of B is less than that with which the speed of B surpasses that of C, B should never rebound, but push C, transferring into it one part of its speed. [...] This can be calculated as follows. If C were exactly twice as large as B, [...] and if B moved more than twice as fast as C, then it should not rebound, *but should transfer as much of its motion to C as is needed to bring it about that they move together afterwards with the same speed.* For example, if C had only two degrees of speed, and B had five (which is more than double), then it should communicate two of the fice, which two being in C would bring about only one [degree of motion] since C is twice as big as B, and thus, afterwards they would both go with three degrees of speed." Translation by Garber, *Descartes' Metaphysical Physics*, 259-60.

described by rules 6, 7c, and 1, there is a balance between resistance and the moving force. Like the previous sets, it involves a scenario in which one body is at rest and the other is in motion, a scenario in which both are in motion toward each other, and a scenario in which both are in motion in the same direction. Whereas I have organized the previous sets according to increasing complexity, this set is organized according to increasing degrees of symmetry. In rule 6 there is symmetry in body, in rule 7c there is symmetry in the quantity of motion, and in rule 1 there is symmetry in both body and quantity of motion.

All three rules are like those in the first set (4, 2, and 7b), in at least one respect: Descartes implies what *would* take place *if* motion were merely transferred. In addition, he indicates what *would* take place *if* motion were not transferred and the bodies merely rebounded. After surveying the possibilities (rebound or transfer), he explains which options conflict with previously established principles, and then presents the remaining outcome.

Because of the equality in rule 6, he claims that there is *no reason* why one or the other of rebound/transfer should occur. In other words, there is no winner in the contest between the force of resistance and the moving force. So, Descartes "splits the difference" between the two hypothetical options of transfer and rebound, and *both* occur.[282] Descartes follows the same reasoning in rule 7c. Rule 1 is similar except that a conflict with a previously established principle decides between the options of rebound and transfer.

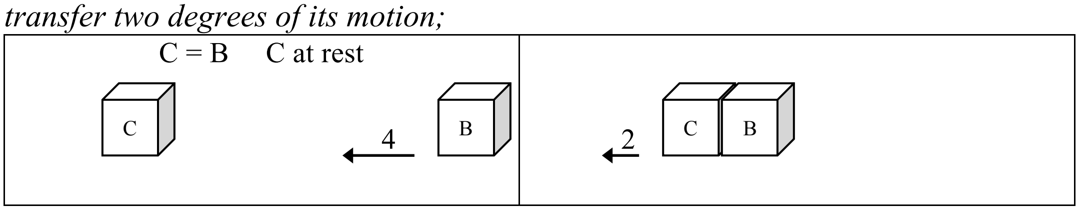
[282] AT IX 92. *Principes* (French) II 51. "It is thus evident that since they are equal, and thus there is no more reason why it should rebound than push C, these two outcomes should be divided equally." Translation by Garber, *Descartes' Metaphysical Physics*, 259.

The numerical examples that Descartes provides for the rules in this set are worth examining in more detail to see Descartes's thought process at work. Rule 6 describes the scenario in which bodies B and C are equal in size. C is at rest and B approaches it. Contrary to other investigations of collision, such as those of Marci and Harriot before Descartes, and Huygens and Wren after him, the bodies do not exchange speeds, with B coming to rest and C moving with the previous speed of B.[283] Rather, for Descartes, B in part transfers motion to C, and in part rebounds from C:

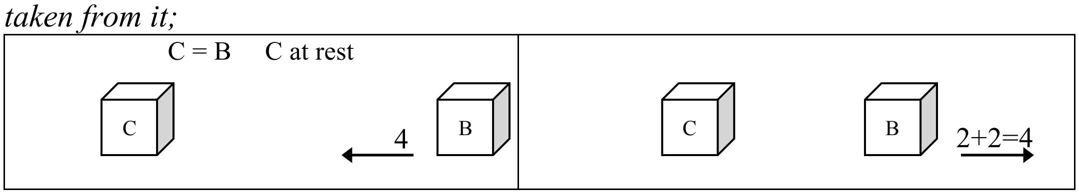
> The sixth [rule] is that if the body C were at rest, and exactly equal in size to body B, which was moving toward it, then it would be necessary that it would in part be impelled by B, and in part it will make it rebound, so that if B went toward C with four degrees of speed, it would be necessary that it transfer one to it, and with the three remaining [degrees] would return in the direction from which it had come.

In order to explain the reasoning of this rule, and to explicate his numerical example (which was merely stated without explanation in the Latin edition of the *Principles*), Descartes goes on to present three possible outcomes complete with numerical examples in the French edition of the *Principles.*

> *Since it is necessary that either B push C without rebounding and thus that it [B] transfer two degrees of its motion;*

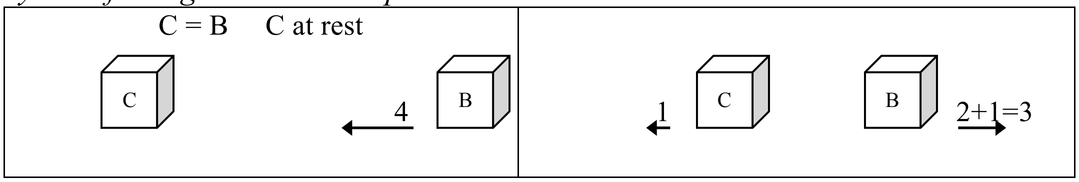


> *or that it [B] rebound without pushing it, and consequently, that it retain these two degrees of speed along with the other two degrees of speed which cannot be taken from it;*



[283] Aiton, "Marci," 157. Sørensen, "Study, part 2," 263." Also see chapters 2, 4, and 5.

or finally, that it rebound and retain one part of these two degrees and push it [C] by transferring to it the other part:



The first possible outcome numerically describes what would take place if motion was transferred. The second describes what would take place if there was no transfer and only rebound. The third divides evenly between the two options. Since there are two degrees of speed that could either be transferred or retained, an even division implies that one degree is retained and the other is transferred. He concludes with his selection among these options:

> *it is thus evident that since they are equal, and thus there is no more reason why it should rebound than push C, these two outcomes should be divided equally. That is, B should transfer to C one of these two degrees of speed, and rebound with the other.*[284]

Because the bodies are equal, there is no winner in the contest between the force of resistance and the moving force. Thus, the first two options can be eliminated.

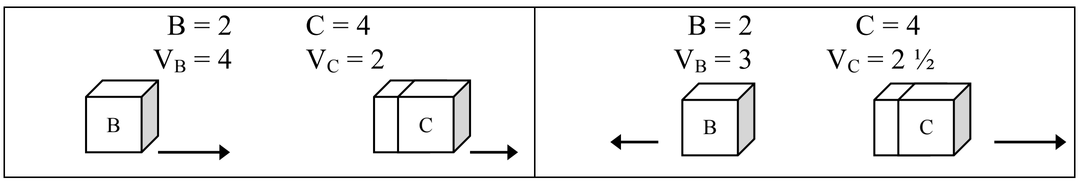
Rule 7c, like 7a and 7b, describes a scenario in which both bodies (which need not be equal in size) move in the same direction, wherein "C precedes but goes more slowly than B." 7a describes the conditions in which the impact will result in a transfer of motion. This occurs when the excess of the speed in B (compared to the speed in C) is greater than the excess of the size in C (compared to the size in B). 7b describes the conditions in which the impact will result in rebound with no transfer of motion. This occurs when the excess of the speed in B is less than the excess of the size of C. Descartes provides numerical examples that illustrated how 7a and 7b can be calculated.

[284] AT IX 92. *Principes* (French) II 51. Translation by Garber, *Descartes' Metaphysical Physics*, 259.

Rule 7c is the scenario in which the excess of the speed in B is equal to the excess of the size of C. Unlike rules 7a and 7b, Descartes did not provide a numerical example to illustrate how the outcome of such a scenario could be calculated.[285] However, it is striking that the rule operates in the same way as rule 6: one part of motion is transferred and the impacting body rebounds with the rest. The text of rule 7c is the following:

> *And finally, when the excess of size which C has is perfectly equal to the excess of speed which B has, the latter ought to transfer one part of its motion to the other, and rebound with the rest.*[286]

Descartes does *not* provide a numerical example, but if he had, it would likely be as follows:



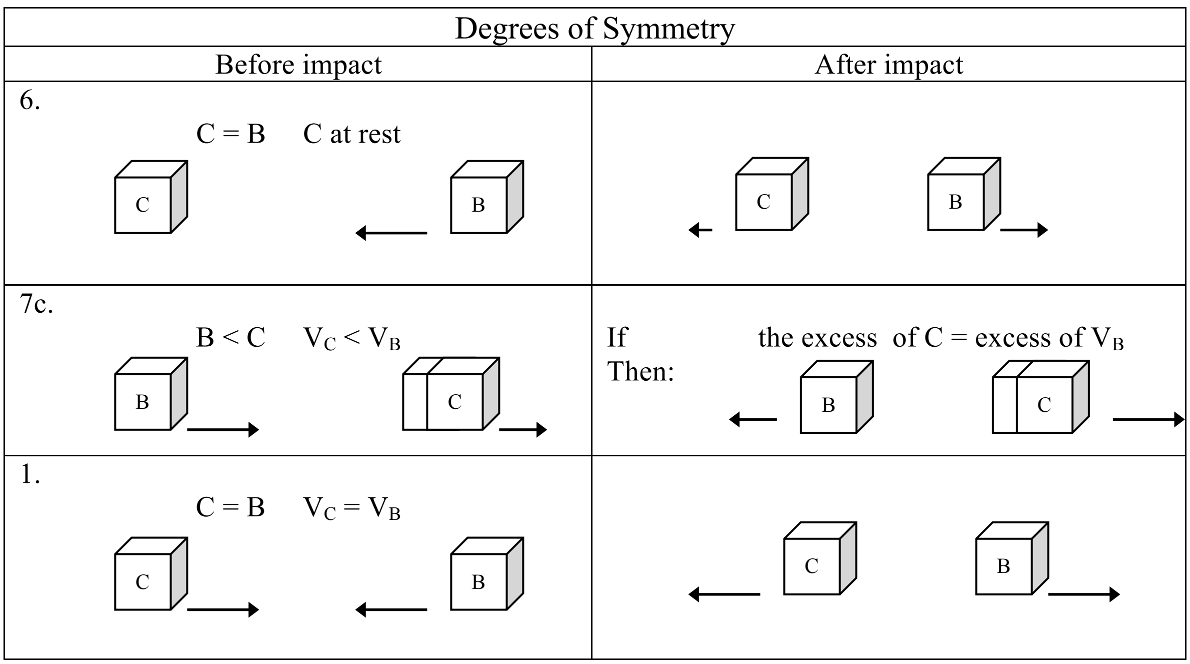
Rule 1, which involves two equal bodies moving in opposite directions towards each other with equal speeds, has the same general structure as 6 and 7c. It too involves an important equality. And he alludes to the various options that are possible—transfer or rebound. However, in rule 1, unlike rule 6 and 7c, Descartes has a means of choosing between the options. Fundamental to his system, and governing each of his rules, is his notion of the conservation of the quantity of motion. In rule 1 the bodies must rebound because the alternative, a transfer of motion, is not possible, given the conservation principle:

> ...when they encountered one another, they would both equally well be reflected, and each would return in the direction from which it had come, without losing any part of its speed, *since in this circumstance there is no cause that can take it [i.e. speed] away, but there is a very evident cause that should force them to rebound;*

[285] Also unlike 7a and b, 7c was not included in the prior Latin edition, although it appears in the French edition of the *Principles*.
[286] AT IX 93. *Principes* (French) II 52. Translation by Garber, *Descartes' Metaphysical Physics*, 259.

and because the cause would be the same for both, they would both rebound in the same way.

Because of the symmetry, if there were a transfer of motion, some part of the speed would be lost (remember, for Descartes, there is no such thing as a negative quantity of motion; motion and the quantity of motion are scalar quantities). If both bodies could transfer their motion to the other in this scenario, they would cease to move. All the speed would be taken away, defying the conservation of quantity of motion.[287] "There is no cause that can take it [*i.e.* speed] away."



5.5 – Analytic method

A pattern of thought running through Descartes's rules of collision is strikingly similar to the classical analytic method, particularly in those rules which stipulate the conditions for rebound, which is the very feature that distinguishes his later account of

[287] Descartes claims that "there is a very evident cause that should force them to rebound. And because the cause would be the same for both, they would both rebound in the same way." Whatever this supposed cause is, it does not evidently follow from his laws of motion and conservation principle. And it is not evident to past commentators, who have noted that rule 1 does not follow from the contest model described in Descartes's impact law.

collision from his early view. Analysis was understood by many to be a method of discovery or problem solving. One assumed that which was sought, and then worked backward to that which was already known. If one could precede successfully back to established propositions, then what is sought could subsequently be demonstrated using the synthetic method, which works step by step through the consequences, beginning with what is known. If, using the analytic method, while working backward, one found a contradiction, the proposition in question was determined to be false.[288] A classic statement of the analytic and synthetic methods has been recorded by Pappus of Alexandria (c.290 - c.350):

> That which is called the *Domain of Analysis*, my son Hermodorus, is, taken as a whole, a special resource that was prepared, after the composition of the *Common Elements*, for those who want to acquire a power in geometry that is capable of solving problems set to them; and it is useful for this alone. It was written by three men: Euclid the Elementarist, Apollonius of Perge, and Aristaeus the elder, and its approach is by analysis and synthesis.
>
> Now, analysis is the path from what one is seeking, as if it were established, by way of its consequences, to something that is established by synthesis. That is to say, in analysis we assume what is sought as if it has been achieved, and look for the thing from which it follows, and again what comes before that, until by regressing in this way we come upon some one of the things that are already known, or that occupy the rank of a first principle. We call this kind of method 'analysis,' as if to say *anapalin lysis* (reduction backward). In synthesis, by reversal, we assume what was obtained last in the analysis to have been achieved already, and, setting now in natural order, as precedents, what before were following, and fitting them to each other, we attain the end of the construction of what was sought. This is what we call 'synthesis.'
>
> There are two kinds of analysis: one of them seeks after truth, and is called 'theorematic'; while the other tries to find what was demanded, and is called 'problematic'. In the case of the theorematic kind, we assume what is sought as a fact and true, then, advancing through its consequences, as if they are true facts according to the hypothesis, to something established, if this thing that has been established is a truth, then that which was sought will also be true, and its proof the reverse of the analysis; but if we should meet something established to be false, then the thing that was sought too will be false. In the case of the

[288] H. J. M. Bos, *Redefining Geometrical Exactness: Descartes' transformation of the Early Modern Concept of Construction* (New York: Springer, 2001), 96-7. Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method* (Cambridge: MIT Press, 2009), 34.

> problematic kind, we assume the proposition as something we know, then proceeding through its consequences, as if true, to something established; if the established thing is possible and obtainable, which is what the mathematicians call "given", the required thing will also be possible, and again the proof will be reverse of analysis; but should we meet with something established to be impossible, then the problem too will be impossible.[289]

Descartes was familiar with the methods of analysis and synthesis. They are clearly on display in his early work on method—the *Regulae ad Directionem Ingenii*, which he worked on throughout the 1620s. Sasaki has argued that his thoughts on developing a method for establishing "the truth of things" were informed by his knowledge of the ancient analysts and the modern algebraists.[290] The natural method he describes is akin to analysis. In rule V of the *Regulae* Descartes wrote:

> We shall be following this method exactly if we first reduce complicated and obscure propositions step by step to simpler ones, and then, starting with the intuition of the simplest ones of all, try to ascend through the same steps to a knowledge of all the rest.[291]

And in rule XVII the similarity with Pappus's statement of analysis and synthesis is even closer:

> So the trick here is to treat the unknown ones as if they were known. This may enable us to adopt the easy and direct method of inquiry even in the most complicated of problems. There is no reason why we should not always do this, since from the outset of this part of the treatise [*i.e.* from Rule XIII, the opening rule of Book Two] our assumption has been that we know that the unknown terms in the problem are so dependent on the known ones that they are wholly determined by them. Accordingly, we shall be carrying out everything this Rule prescribes if, recognizing that the unknown is determined by the known, we reflect on the terms which occur to us first and count the unknown ones among the known, so that by reasoning soundly step by step we may deduce from these all the rest, even the known terms as if they are unknown.[292]

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289 Pappus of Alexandria, *Book 7 of the Collections*, *Part 1*, edited and translation by Alexander Jones (New York: Springer, 1986), 82-3.

290 Sasaki, *Descartes*, 92, 178. Sasaki builds his argument, in part, from Descartes's autobiographical statements in the *Regulae ad Directionem Ingenii* and *Discours de la méthod*.

291 AT X 379. Translation by Sasaki, *Descartes*, 183.

292 AT X 460-1. Translation by Sasaki, *Descartes*, 183-4.

Descartes allegedly revealed his "natural method" in Paris at a meeting at the palace of the Papal Nuncio in 1627/8, which may have been the first public recognition of Descartes's philosophical ideas.[293] Although Descartes abandoned the *Regulae* project in 1628, he continued to refer to analysis. For example, in part II of the *Discours de la méthod* (1637) he mentioned it, although in doing so he disparaged the material to which the analysis of the ancients was applied and the cumbersomeness of the algebra of the moderns:

> [They] cover only highly abstract matters, which seem to have no use. Moreover the former is so closely tied to the examination of figures that it cannot exercise the intellect without greatly tiring the imagination; and the latter is so confined to certain rules and symbols that the end result is a confused and obscure art which encumbers the mind, rather than a science which cultivates it.[294]

The *Discours* is accompanied by three essays, the *Dioptrics, Meteors, and Geometry.* In the latter, Descartes famously used his symbolic *algebraic* analysis to solve geometrical problems. The central problem that Descartes solved in the *Geometry* was one that had been proposed to him in the early 1630s by Jacobus Golius, a professor of mathematics at the University of Leiden.[295] This was a locus problem, often referred to as "Pappus' Problem," since it is found in Book VII of Pappus's *Collection*, specifically in Pappus's commentary on Apollonius's *Conics*.[296]

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[293] Sasaki, *Descartes*, 176-7. Gaukroger, *Descartes Biography*, xvi, 183-6. There is some disagreement regarding the date of the meeting in Paris.

[294] AT IV 17-18. Translated by Sasaki, *Descartes*, 15-16. Garber has argued that Descartes had essentially abandoned investigations of "method" altogether by the time he published *Discours de la méthode*, in favor of the "proper order of inquiry." "In claiming that Descartes' later works do not display his earlier method I am making a controversial claim, one that would be challenged by other scholars, who have claimed to find the method of the *Rules* and *Discourse* in the *Meditations*, at very least." Garber, *Descartes' metaphysical physics*, 48-9.

[295] Golius filled the position of Willebrord Snel upon his death. Recall that Snel had been an important figure in Isaac Beeckman's mathematical education. See chapter 2.

[296] Sasaki, *Descartes*, 206-7.

Although in the intervening years the topics about which Descartes wrote changed, he continued to at least allude to analysis as a general method for solving problems (not restricted to mathematical domains, and not restricted to *algebraic* analysis specifically). For example, in the second set of replies for the *Meditations* Descartes again describes the difference between an analytic and synthetic method, and states that "it is analysis which is the best and truest method of instruction, and it was this method alone which I employed in my *Meditations*."[297]

Clearly, Descartes was familiar with the analytic method. It was important for his early thoughts on natural method. It was also of immense importance to his analytic geometry, which would have a profound impact on the development of mathematics. However, I am not arguing that Descartes's contributions to mathematics had an impact on his rules of collision. It was, apparently, not obvious to express collision algebraically, and he kept these domains quite separate. As we will see in chapter 4, Huygens began to make connections between symbolic algebra and collision, and Wren would combine them closely. I am also not arguing anything about the extent to which Descartes continued to be interested in method after his so-called "metaphysical turn" in 1628.[298] Nor am I arguing that the *Principles of Philosophy* should be understood as a treatise on method. It is not. With the exception of Marcus Marci, no one prior to Descartes had published an account of the rules of collision. This was a new problem. As such, we see an expression of the analytic method being used by Descartes as a means to solve this

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[297] René Descartes, "The Author's Replies to the Second Set of Objections," in *The Philosophical Writings of Descartes: Volume 2*, trans., John Cottingham, Robert Stoothoff, and Dugald Murdoch (New York: Cambridge University Press, 1984), 110-11.
[298] Sasaki, *Descartes*, 203.

new problem. This is what I'm claiming: Descartes used a problem-solving method, with which he was quite familiar, to solve a new problem.

Descartes's impact law, which can be found in both his early and later views of collision, described both the transfer of motion and rebound.[299] Rebound occurs when the force of resistance is greater than the moving force, and is thus not overcome. However, in Descartes's early view, he never stipulates when the force of resistance is actually greater. In each numerical example that he provided, a transfer of motion occurs, whether it was regarding phenomenal bodies such as stones moving the earth, or two bodies described in abstraction. What is new to the rules of collision in the *Principles of Philosophy* is that he stipulates the conditions in which the force of resistance is not overcome. This is the new problem Descartes faces in the *Principles*, and it is with these scenarios of rebound (in rules 4, 2, and 7b, as well as rules 6, 7c, and 1) that we see the analytic method most clearly. He presents the various possible outcomes, and then works backwards from each. Those instances in which he finds a contradiction with a previously held principle are eliminated as a possible outcome.

In rules 4, 2, and 7b, a transfer of motion *would* take place, *if not for* the force of resistance prevailing in the contest. The key condition in each of these scenarios is *size.* In rule 4, Descartes outlines the two possible options: a transfer or motion, or no transfer of motion resulting in rebound. He provides several numerical examples of the transfer of motion, which would proceed in the same way that he described in his early view, and would do so in accordance with the laws of nature and conservation of quantity of motion. However, as we have seen above, he has selected *size* as the key condition that determines when the force of resistance is not overcome. The body at rest is larger than

[299] See sections 4.4 and 5.2.

the moving body. Thus, the force of resistance is not overcome, and transfer of motion is not possible.

In rules 6, 7c, and 1, there are balances of forces. Here again, Descartes presents the various possible outcomes, rebound or transfer: he implies what *would* take place *if* motion were merely transferred; and he indicates what *would* take place *if* motion were not transferred and the bodies merely rebounded. Working backward in a similar manner as rule 4 above, he attempts to eliminate one or the other possible outcomes. However, as we have seen in rules 6 and 7c, there is no way to choose between the two possible outcomes, and he splits the difference. In rule 1, on the other hand, one option (transfer of motion) results in a contradiction, since it would conflict with the established principle of the conservation of quantity of motion. Thus, in rule 1, both bodies rebound.

There are several differences between the rules of collision in the Latin and French editions of the *Principles of Philosophy*. The order of the rules, and the outcomes of the rules remain the same across the editions. An important change is the amount of explanation Descartes provides. In the Latin edition, Descartes did not present rule 4 together with a statement of what would occur if motion were transferred—similarly, with rule 6. In the Latin, he merely states what would occur. In the French, the same outcome would occur, but he has also provided the examples of what would occur if motion were merely transferred or if no motion were transferred and the bodies rebounded, with the added explanation for why he splits the difference between the options. This is what one would expect with the analytic method. After he was compelled to explain himself more thoroughly to Clerselier upon the publication of the Latin edition, Descartes provided additional instruction in the French edition of the *Principles*. The explanations reflect the analytic method. This method was understood by many of the moderns to have been used by the ancients to solve their problems but that the ancients also suppressed the method in their demonstrations. Descartes himself mentions this in his *Regulae*[300] and the Replies to the Second Set of Objections to the *Meditations.*[301] Descartes had described the analytic method to be particularly suited to instruction, which is the role in which he would have found himself—instructing his readers on the proper understanding of his rules.

5.6 – Force of resistance

Descartes was not clear in what he meant by the force of resistance. Most commentators have assumed that what Descartes meant to say, but did not say, was that the force of resistance is the product of the size of the body and the speed of the body. However, Descartes describes three scenarios in which one body is at rest, and yet the body at rest has a force of resistance. This seems to conflict with the presumed interpretation of force of resistance as the product of body and speed, since the force of resistance of a body at rest under this interpretation would be nil. The attempt to resolve this issue has been to claim that the force of resistance is the product of the body at rest and the speed of the body *approaching*, which is a variation of the force of resistance of a

[300] AT X 373, 376-7. "[W]e are well aware that the geometers of antiquity employed a sort of analysis which they went on to apply to the solution of every problem, though they begrudged revealing it to posterity." Translation by Cottingham et al., *PW* I, 16-17. "They may have feared that their method, just because it was so easy and simple, would be depreciated if it were divulged; so to gain our admiration, they may have shown us, as the fruits of their method, some barren truths proved by clever arguments, instead of teaching us the method itself, which might have dispelled our admiration." Translation by Sasaki, *Descartes,* 91.

[301] AT IX 122. "It was synthesis alone that the ancient geometers usually employed in their writings. But in my view this was not because they were utterly ignorant of analysis, but because they had such a high regard for it that they kept it to themselves like a sacred mystery." Translation by Cottingham et al., *PW* II, 111.

moving body, which is akin to the quantity of motion. Proponents of this interpretation cite selected passages from rule 4 and Descartes's elaborations in the letter to Clerselier for support, primarily noting that some of Descartes's descriptions of the force of resistance in a body at rest lend themselves to an interpretation that the force is variable. Several theses have relied on this aspect of the interpretation—that the force of resistance in a body at rest varies in proportion to the impressed force—such as Gabbey's argument regarding Descartes's notion of force, and Garber's account of the development of Descartes's rules from the Latin edition to the Letter to Clerselier to the French edition of the *Principles*. I argue that this interpretation is not what Descartes intended. (1) Descartes did not say that this is what he meant. (2) If one does not impose a mathematical form that Descartes himself refrained from using, then there is no difficulty to be explained, such as the non-zero value of the force of resistance of a body at rest. In other words, the above interpretation is an *ad hoc* solution to a problem which itself is an artifact of rewriting Descartes's ideas in mathematics that Descartes did not use. (3) Using the above interpretation, which was designed to solve a specific problem that is produced when one relies on a symbolic algebraic reconstruction of Descartes's ideas, one runs into further obstacles with Descartes's statements in the letter to Clerselier in which Descartes attempted to explain his rules to his friend. Regarding the statements made by Descartes which seem to lend themselves to an understanding of the force of resistance in a body at rest as variable, I will show that these same passages make just as much sense when one interprets the force of resistance in a body at rest as absolute.

In 1957, E. J. Aiton provided the account of the force of resistance in a resting body that has become the received view. Aiton’s interpretation, or at least interpretations quite similar to his, can be found in most subsequent commentators on the topic including Gabbey and Garber. The role of “the approaching speed” has been reaffirmed by many, some of whom use different mathematical expressions. The kernel of the interpretation has then persisted in subsequent accounts. The following is Aiton’s argument reproduced in full:

> An answer to the question of the nature of the “force” of a body to resist the action of another is suggested by the secondary rules. For a body in motion, this “force” is the quantity of motion (*mv*), as is evident from 7(a) and 7(b), where the “force” to resist of m_2 is greater or less than the “force” of m_1 according as m_2v_2 is greater or less than m_1v_1. For a body at rest, however, as is evident from rule 4, the “force” to resist is equal to the quantity of motion the body would need to acquire in order to move with the original speed of the body which strikes it. Thus the “force” to resist is m_2v and this is greater than m_1v provided m_2 is greater than m_1. An interesting point is that the “force” to resist is not constant but depends on the impressed force. Descartes’s conception of the “force” of a body at rest to resist motion, increasing in direct proportion to the impressed force up to a certain limit, after which motion takes place, indicates a sound intuition of the situation involved in moving heavy objects. Uncritical reliance on common experience, however, led him to locate this resistance in the body itself instead of finding it in the friction between the body and the others in contact with it.[302]

In those scenarios in which both bodies are in motion (such as 7a and 7b) the forces of resistance are interpreted as the quantities of motion, which Aiton expresses as the products of body *m* and speed *v*, m_1v_1 and m_2v_2 respectively. The force of resistance in a resting body is then expressed analogously to the quantity of motion represented as an algebraic project. For example, if m_2 was the resting body, the body would obviously have no speed. But Aiton seems to assume that the force of resistance in a resting body must be similar to the quantity of motion, which for Aiton is a product of body and speed. So he uses the speed of the body *approaching*. The force of resistance in the resting body becomes m_2v_1.

[302] Aiton, "Vortex Theory," 253-4.

One very nice feature of Aiton's interpretation is that it is consistent with rule 4. Garber, who holds an interpretation akin to Aiton's, explains the situation. The force of resistance in a resting body (C) is proportional to the speed of the body *approaching* it. In rule 4, recall, if C > B, no matter how fast B approaches C, B will rebound and will not move C. According to Garber:

> on Descartes' conception, the force of resisting in C is to be measured by the product of m (C) and v (B). And so, however quickly B were to move, it would always be reflected by a larger C, and, conversely, however slowly B might move, it would always be able to set a smaller C into motion.[303]

This idea may have seemed initially plausible because of the similarity to the algebraic expression Aiton used for quantity of motion, and the assumption that Descartes's notion of force must unify the quantity of motion, the moving force, the force of resistance of a moving body, and the force of resistance of a body at rest. Descartes never used an algebraic expression for the quantity of motion (and therefore did not subsequently presume that the same algebraic expression would be used for the force of resistance in a body at rest). Descartes's notions of force are famously unclear and problematic, as noted in section 2.3 as well as in appendix 1. I disagree with Aiton's interpretation. I think it is probable that Aiton's interpretation was due to his over-reliance on a form of mathematical expression that Descartes never used to represent quantity of motion, *mv.*

Alan Gabbey has also supported a variation of this interpretation, prior to Garber. The existence of a force of resistance in a resting body shows, according to his argument, that "the forces involved in collisions do not arise exclusively from motions or from determinations,"[304] in other words, the ontology of force is not reducible to matter and motion. Rule four, according to Gabbey is not a "Cartesian aberration," but rather "the

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[303] Garber, *Descartes' Metaphysical Physics*, 240.
[304] Gabbey, "Force and Inertia," 266.

most seminally valuable of the seven [rules]."[305] It reveals the ontological status of force, and is a major component in Gabbey's argument of the place of Descartes's thought as midway between Newton's conception of force in Newton's third law and the "traditional" understanding of force rooted in the "contest view."[306] Gabbey presents the measure of this force "in terms of the motion that the striking body 'tries' to impart to it."[307] He claims that "Descartes quantifies the resisting force by setting it *equal to the total change in motion* (or equivalently determination) that it would receive..."[308] Gabbey provides an algebraic reconstruction of rule four under this interpretation.[309] As proof texts for this view, Gabbey quotes the fourth rule and the lengthy (expanded) explanation Descartes provided in the French edition of the *Principles* in their entirety, as well as a large portion of the 1645 letter to Clerselier in which Descartes further explains himself on this topic to his correspondent. Presumably, Gabbey thought his position was evident given these texts.[310]

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305 Ibid., 269.
306 Ibid., 244-5.
307 Ibid., 268.
308 Ibid.
309 Ibid., 268-9. "In the case of Rule Four, the bodies B and C, being perfectly hard and therefore perfectly inelastic, *would* move with the same final speed $(B \times V_B)/(B + C)$ and quantities of motion $(B^2 \times V_B)/(B + C)$ and $(C \times B \times V_B)/(B + C)$ (respectively) *if* B were to move C. But C at rest resists the reception of the quantity of motion $(C \times B \times V_B)/(B + C)$ with a force of equal size, and this force is contested by the *remaining* motive force of B, given by $(B^2 \times V_B)/(B + C)$. Hence (by the Third Law), B will not move C, since: $(C \times B \times V_B)/(B + C) > (B^2 \times V_B)/(B + C)$ given that C > B. Alternatively, since B and C would move with the *same* final speed if B were to move C, one can say that C resists with as many 'degrees of resistance' as B has degrees of speed, and therefore the excess of force will depend only on the relative sizes of the bodies. Hence, as before, B will not move C. Here Descartes seems to be considering the possibility of a measure for rest which will be analogous to the speed that measures one dimension of the active force of motion."
310 Peter McLaughlin has also followed the interpretation that the force of resistance in a resting body is measured by the speed of the body approaching it. However, he moves away from algebraic expressions, although the root of the interpretation is still present. McLaughlin uses the balance as a way to express Descartes's mathematics and to support his arguments regarding Descartes's supposed use of symmetry conditions to develop his rules of collision. He claims that in 17th century thinking, the best way to understand which force is stronger in a contest (model of force) is to "put them on a scale," so to speak—in fact, "most seventeenth-century discussions of impact take their cue from statics and the law of the lever. [...]The application of statics to collision is fairly straight-forward when both bodies are in motion and only

This interpretation, that the force of resistance in a body at rest is the product of the body and the speed of the body approaching, was initially designed as a solution to a problem that was an artifact of rewriting Descartes's ideas in mathematics that Descartes did not use. If one does not impose a mathematical form that Descartes himself refrained from using, then there is no difficulty to be explained. Descartes did not explicitly claim that the force of resistance was the product of body and speed. As we have seen, in his early view Descartes did not stipulate the force at all, and in every instance the force of resistance was overcome by the moving force, even though according to the early versions of his impact law the force of resistance in principle could be larger than the moving force and thus prevail in the contest. In his later view Descartes stipulated when the force of resistance is greater. The primary scenario in which the force of resistance is not overcome is rule four: a smaller moving body reflects off a larger body at rest, transferring no motion. As he explained to his friend Clerselier, "without this [rule 4], no body would ever be reflected by encountering another."[311] In other words, if not for this, collision would proceed as he had described in his early view, and as Beeckman had described before him—transferring motion with every impact. As seen in section 5.4.1

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the determinations are opposed, as in Descartes' first three impact rules. In these cases he treats the size and speed (distance per unit time) of a body just like weight and the length of the balance arm. Difficulties with the statics model arise for Descartes when he moves to the opposition between motion and rest. If one body is at rest, how do we represent the quantity of its rest? Descartes stipulates strict symmetry between the two contrary modes, motion and rest: If the velocity of one body is represented by the length of a lever then the quantity of rest of the other body is represented by a lever of the same length. For equal bodies one has as much rest as the other has motion. In an opposition between rest and motion the bigger body is always the stronger." Mclaughlin claims that there is "strict symmetry" between motion and rest. But ultimately his explanation is very similar to those of Garber, Gabbey, and Aiton. The body at rest has a strength of resistance that is proportional to the size of the body at rest and the speed of the body approaching. McLaughlin calls it a "quantity of rest," which is represented by a body situated on a balance arm, which is the same length as that of the balance arm representing the speed of the moving body. See Peter McLaughlin, "Force, determination and impact," in *Descartes' Natural Philosophy*, ed. Stephen Gaukroger, John Schuster, and John Sutton (New York: Routledge, 2000) 98-9.

[311] AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.

rules 2 and 7b are extensions of rule 4 to increasingly complex scenarios, but in all of which the force of resistance is greater than the moving force.

The interpretation that the force of resistance in a body at rest is the product of the body and the speed of the body approaching conflicts with the explanations that Descartes provided in his letter to Clerselier. Using the received view of the force of resistance in a body at rest, Garber, for example, cannot make sense of an otherwise clear claim regarding the impact law and the importance of size for the force of resistance. As we will see in the following section 5.6.1, Descartes clearly articulates *size* as the relevant condition for the force of resistance in a body at rest (and not the product of body at rest and the speed of body approaching). He explains this in two different ways. (1) He uses the familiar inverse proportion of bodies and speeds before and after collision which Beeckman had used and Descartes had used in his early view, but expressed in terms of the possible transfer of up to half of the speed of the moving body. This is just an alternate way of stating that a body smaller than the resting body transfers no motion and rebounds. (2) He also uses the so-called Principle of Least Modal Change (PLMC) to highlight the size of the body as the determining condition in transfer and rebound.

Advocates of the above received view rely on a particular reading of portions of the Latin and French editions of the *Principles* and the letter to Clerselier. Namely, they rely on statements that seem to lend themselves to an understanding of the force of resistance in a body at rest as variable. For instance, Garber supports his interpretation of the force of resistance in a resting body by quoting from Descartes's explanation of rule 4 from the Latin *Principles of Philosophy,* which Garber himself admits is "by no means lucid:"

> [A] resting body resists a greater speed more than it does a smaller one, and this in proportion to the excess of the one over the other. And therefore there would always be a greater force in C to resist, than there would be [a force] in B to impel.[312]

Garber further defends his interpretation with a passage from the letter to Clerselier in which Descartes provides some explanation of rule 4 to his friend. "[I]f it [the body at rest] is moved by a body which moves twice as fast as an another, it ought to receive twice as much motion from it; but it resists twice as much this twice as much motion."[313] And a similar passage appears in the French edition:

> *But since C is at rest, it resists the reception of twenty degrees ten times more than it resists the reception of two, and it resists the reception of two hundred one hundred times more, so that the more speed B has, the more resistance it finds in C.*[314]

Some infer that the force of resistance in a body at rest must be variable because of statements such as these: "[a] resting body resists a greater speed more than it does a smaller one," and "[the body at rest] resists twice as much this twice as much motion," and "the more speed B has, the more resistance it finds in C."

It is more likely that the passages from the letter to Clerselier and the French edition of the *Principles* are illustrations of Descartes's chosen condition, as specified in rule 4, for rebound—size. As Descartes explained in the French edition of the *Principles*: "*its resistance [i.e. the resistance of the larger body at rest] ought to prevail over the action of B because it is larger than it.*"[315] The above passages, which seem to imply that the force of resistance is variable, make just as much sense if we trust Descartes's words

[312] AT VIII 68. *Principia* (Latin) II 49. Translation by Garber, *Descartes' Metphysical Physics*, 240. In my view, here Descartes seems merely to be stressing that a larger body at rest will resist no matter what. See my argument below.
[313] AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 243.
[314] AT IX 91. *Principes* (French) II 49. Translation by Garber, *Descartes' Metaphysical Physics*, 257.
[315] Ibid.

that size is in fact the condition that is relevant. If the force of resistance, determined by the size of the body, is absolute, then the speed of the body approaching is irrelevant.[316] It will resist any speed, a great speed or a small speed. Since the greater speed offers more moving force than a smaller speed, the body at rest, in a sense, "resists a greater speed more than it does a smaller one," or "the more speed B has, the more resistance it finds in C." But the force of resistance in the body at rest resists absolutely because it is larger in size than the smaller body moving toward it. Because of the size, no motion is transferred. No matter how much speed the moving body has, it will never overcome the force of resistance. The moving force will not overcome the force of resistance if it moves twice as fast, or twenty times as fast, or one hundred times as fast.

Descartes did not state that a larger resting body has a force of resistance that depends on the degree of speed of the body moving toward it, such as m_2v_1. I contend that this interpretation was the result of a solution to an artificial problem that was produced from rewriting Descartes's ideas in a mathematical form that Descartes's did not use. If one refrains from using this mathematical form, then there is no problem to be solved. The received view also conflicts with Descartes's explanations of rule 4 in his letter to Clerselier. Descartes repeatedly claims in a variety of ways that the force of resistance in a body at rest depends on the size of the bodies. Speed is irrelevant. Those passages regarding the force of resistance of a larger body at rest that have frequently been

[316] In the letter to Clerselier Descartes explains that speed is irrelevant—that the contest between the moving force and the force of resistance depends only on size—by claiming that the body at rest "has as many degrees of resistance as the other, which is moving, has of speed," which is another way of stating that speed is inconsequential. See AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.

interpreted as the force as variable are consistent with an understanding of the force as absolute.

5.6.1 – Letter to Clerselier

After Descartes published the Latin edition of the *Principles of Philosophy* he was called upon to explain himself to his friend Clerselier regarding rule 4 as well as the other rules involving one body at rest. Previous commentators have found principles in the letter that do not seem to appear in the Latin *Principles*, such as the so called *Principle of Least Modal Modification* (PLMM) also known as the *Principle of Least Modal Change* (PLMC). Garber has provided a detailed account of the development of Descartes's understanding of collision from the Latin *Principles* through the Letter to Clerselier to the French edition of the *Principles*. In Garber's telling, Descartes introduced several new principles in the letter, such as the so-called "sophisticated impact contest," the PLMC, and a symmetry principle, but never fully brought the project to fruition in the French edition. Instead the French edition was a hybrid of ideas, but ultimately the rules of collision remained an unfinished draft.[317] Although Descartes's overall project is problematic, his rules are not as incomplete as Garber suggests. The root of the problem has been that the letter has been read through the interpretive lens of the received view of the force of resistance of a body at rest. When this interpretive tool is set aside, we see that Descartes's rules do not seem as problematic and incomplete. Descartes was not

[317] Garber, *Descartes' Metaphysical Physics*, 231, 242, 252. "Descartes' full treatment of impact was late in coming and, in the end, never fully worked out; the record shows him struggling with the problem, and never really arriving at a single satisfactory view on the problem. ... Indeed, Alan Gabbey has made a very convincing case for the claim that the seven rules are a late addition to the *Principles*, and were hastily added only after what is now Pr II 45 and Pr II 53 were fully drafted. ... The question of impact seems to remain an unsettled question up until the end of Descartes' life, work in progress that he never quite finished."

testing out multiple implicit principles in the various texts. Rather, Descartes's later ideas remained largely the same. By the time he wrote the *Principles*, his ideas had moved away from his "early view." He had developed and determined his rules, which remain the same through all the texts of his "later view," in regards to their order and outcomes and rationale. Descartes was not introducing new principles in the letter, and presenting a hybrid in the French. Rather, he was explaining the same principle in several different ways to try to convince his friend Clerselier.

The letter Clerselier sent to Descartes does not survive. However, from Descartes's response we see that Clerselier had likely asked why it is "that a body without motion would never be moved by another smaller than it, whatever the speed with which this smaller body could move."[318] He asked about rule 4. Throughout the course of the letter, Descartes responds to this question in three different ways. In the first paragraph, he appeals to the impact law, with an emphasis on size. In the second paragraph, he uses a numerical example to illustrate the contest view where the determining condition is the *size* of the body at rest, but he expresses it in a slightly different way from either the Latin or French edition. Nevertheless, it is not substantially different. In the third paragraph, he says that everything relies on one principle, the "principle of least modal change," which he explains in the fourth paragraph. His explanation is connected to the numerical proportions in the second paragraph. His explanation also reveals that the PLMC is just another way of describing the contest between resistance and the force of moving.

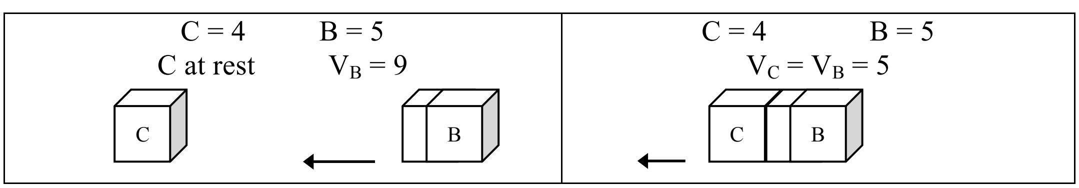
[318] AT IV 183-4. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 260-1.

In the first paragraph, Descartes responds to Clerselier's question by referring him to the impact law, with an emphasis on *size* as the relevant condition for overcoming the force for moving, resulting in rebound with no transfer of motion:

> [I]t is a law of nature that a body that moves another must have more force to move it than the other has to resist it. But this greater [force for moving] can depend only on its size, for that which is without motion has as many degrees of resistance as the other, which is moving, has of speed. The reason for this is that if it is moved by a body which moves twice as fast as another, it ought to received twice as much motion from it; but it resists twice as much as this twice as much motion.[319]

Note that Descartes clearly states that it "can depend only on its size."[320] In the second paragraph, Descartes explains this very principle with some contrasting examples. He begins with an example in which motion is transferred to a body initially at rest, and then contrasts this with an example in which motion cannot be transferred to a body initially at rest:

> For example, body B cannot push body C without making it move as fast as it itself would move after having pushed it. In particular, if B is to C as 5 is to 4, with B having 9 degrees of motion, it must transfer 4 of them to C to make it go as fast as it goes. This is easy, for it has the force to transfer up to four and a half (that is, half of what it has) rather than reflecting its motion in the other direction.[321]



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[319] AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.

[320] Garber claims that the above quoted passage from the letter to Clerselier is merely a restatement, without argument, of the notion that "the force of resisting in a resting body is proportional to the speed of the moving body." Garber, *Descartes's Metaphysical Physics*, 243. However, we clearly see the same pattern of thought described above. Descartes describes the possible outcomes, such as what would occur if motion were to be transferred: "if it is moved by a body which moves twice as fast as another, it ought to receive twice as much motion from it." But motion is not transferred, because the resting body is larger. Therefore the resting body will resist being moved, no matter what speed the moving body has.

[321] AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.

This is exactly the same transfer of motion that we have seen in rule 5, as well as all throughout Descartes's early view, as well as the accounts in Beeckman's *Journal* before him. There is an inverse proportion between the size of the moving body before collision and the larger moving body after collision, to the speed of the initially moving body and the speed of the larger moving body after collision.

$$\frac{\text{body B of size } 5}{\text{body B} + \text{C after collision of size } 9} = \frac{\text{speed of body (B} + \text{C) after collision } x}{\text{speed of body B before collision } 9}$$

$$\frac{5}{9} = \frac{x}{9}$$

$x = 5$ (speed of body B+C after collision)

The larger body (both bodies moving together) must move 5 degrees of speed. Thus 4 degrees of speed must have been transferred in the collision.

The *explanation* that Descartes provides in the letter appears novel (and arbitrary)—"it has the force to transfer up to four and a half (that is, half of what it has) rather than reflecting its motion in the other direction." But it is really just another way of stating that a body will transfer motion to another body at rest of any size up to but not including a body equal to itself. In other words, it is another way of stating that *size* is the relevant condition. The reasoning is as follows:

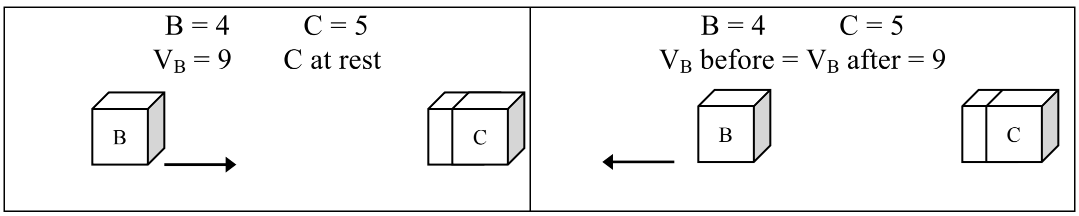
$$\frac{\text{body B of size } 5}{\text{body B} + \text{C after collision of size } 5 + y} = \frac{\text{speed of body B} + \text{C after collision up to } 4\frac{1}{2}}{\text{speed of body B before collision } 9}$$

$y = 5$ (size of body B+C after collision)

In other words, the explanation of Descartes's numerical example claims that a moving body will transfer motion to a body at rest as long as that body is less than equal its size. Descartes has merely restated the same principle mentioned in the first paragraph, the impact law, with emphasis on the condition of size. Using the same familiar inverse proportion that he has always used in cases of the transfer of motion, he has expressed the principle in terms of the speeds involved rather than the bodies.

Descartes then contrasts this with another numerical example in which no motion is transferred:

> But if B is to C as 4 is to 5, B cannot move C, unless of its nine degrees of motion it transfers 5 to C, which is more than half of what it has, and which, as a consequence, C resists more than B has the force to act.[322]



Descartes's explanation in this example is the same. A body can only transfer up to half of the motion it has to a body at rest. In other words, a body can only transfer motion to a resting body that is up to equal its size. In this case, it would transfer 5 degrees of motion, and both would move together with 4 degrees of speed. But this is impossible because 5 degrees of motion is more than half of the 9 degrees of speed body B has. Or, in other words, body C is more than equal to B in size. Thus, the force of resistance is not overcome, and no motion is transferred.

Gabbey and Garber interpret this differently. They see the force of resisting in the body at rest to be measured by "the quantity of motion it *would* have if B *were* to succeed in pushing it after the collision, then the faster B goes, the more resistance C offers."[323] Garber calls this the "sophisticated impact contest model" as opposed to the "simple impact contest model" found in the Latin *Principles*. "On this view, force is not proportional to *motion* but to *change of motion,* the quantity of motion that a resisting

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[322] AT IV 184-5. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.
[323] Garber, *Descartes' Metaphysical Physics*, 244. Gabbey, "Force and Inertia," 269-70.

body *would* have if it *were* set into motion by collision."[324] Gabbey has argued that "this conception of force of rest also looks forward to the Newtonian conception of force in terms of change of motion."[325] Gabbey and Garber have misinterpreted this passage. The conditional language Descartes uses is due to his method of presenting the possible outcomes, and working backward to find a conflict. It is a mistake to conflate it with the so-called measure of the force of resisting. Descartes was not "looking forward" to Newton. Moreover, using the received view as an interpretive lens, Garber cannot make sense of the numerical examples Descartes provides in the second paragraph.[326]

Descartes closes paragraph 2 by reiterating that he has explained why the smaller body must rebound upon meeting a larger body at rest.[327] Moreover he states: "And without this, no body would ever be reflected by encountering another." Rule four is of primary importance for an account of rebound. Without it, there would be no rebound.

In the third paragraph, Descartes attempts to clear away any more difficulties Clerselier might have with his rules. To do so, he asks Clerselier to note that all of the rules "depend only on a single principle, which is that

> when two bodies having incompatible modes collide, there must really be some change in these modes, in order to render them compatible, but that this change is always the least possible, that is, if they can become compatible by changing a certain quantity of these modes, a greater quantity of them will not be changed. And we must consider two different modes in motion *[mouuement]*: one is motion *[motion]* alone, or speed, and the other is determination of this motion *[motion]* in a certain direction, which two modes change with equal difficulty.[328]

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[324] Garber, *Descartes' Metaphysical Physics,* 244.
[325] Garber, *Descartes' Metaphysical Physics,* 244. Gabbey, "Force and Inertia," 243-72.
[326] Garber, *Descartes' Metaphysical Physics*, 244. "[T]here is still a deep arbitrariness in Descartes' account: I can see no argument for the claim he makes in paragraph 2 that a body in motion has force enough to impose only half of its motion on another; this is not obvious in itself, nor does it follow from anything he says either in this paragraph of the Clerselier letter or in the Latin *Principles*."
[327] AT IV 186. Descartes to Clerselier, 17 February 1645. "This is why B ought to rebound in the other direction instead of moving C." Translation by Garber, *Descartes' Metaphysical Physics*, 261.
[328] AT IV 185. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.

This principle (known as the PLMM or PLMC), taken as a new and independent principle, does not appear significantly in the Latin or French *Principles*.[329] It seems to be a significant departure from the impact law. The PLMC has been discussed extensively among commentators on Descartes's physics, covering topics such as the apparent lack of forces in the principle, its teleological nature, its relationship to the impact law, and the priority of the principle in Descartes's system.[330] I will not take up these discussions except one. Garber has claimed that the PLMC "seems to bear no connection to the impact contest model of law 3," nor is it closely related to the previous paragraphs in the letter.[331] Although the PLMC may be problematic, the explanation that Descartes provides for it in paragraph four bears a striking resemblance to the impact law, with the specification that *size* is the relevant condition determining whether rebound, transfer of motion, or both occur upon impact. Moreover, we will see that the PLMC is not only related to the rest of the letter, but that all three explanations Descartes provided in the first four paragraphs of his letter are closely bound together.

In rules 4, 5, and 6, one body is at rest and one is in motion. In other words, their modes are incompatible. In a statement that resembles the impact law, Descartes claims that:

> they can become compatible in two ways, namely, if B changes the entire determination of its motion, or if it changes body C's rest, transferring to it such part of its motion as will enable it [B] to push C in front of it as fast as it will itself go.[332]

[329] Garber, *Descartes' Metaphysical Physics*, 247, 251. However, Garber claims that there is a "clear reference to it in the explanation of R3" in the French *Principles*.
[330] Garber, *Descartes' Metaphysical Physics*, 247. Gabbey, "Force and Inertia," 264-5.
[331] Garber, *Descartes' Metaphysical Physics*, 247.
[332] AT IV 186. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 262.

The first way is for B to change its determination/direction. The second way is if B transfers the requisite amount of motion such that they will move together after impact. These are the two options Descartes outlines in his impact law. If this were a restatement of the impact law, we would expect that Descartes would outline the possible outcomes of the contest. And, since one body is at rest, its force of resistance in the contest is determined by *size*. This is precisely what Descartes does in the next sentence:

> And I have said nothing in these three rules but this, that when C is larger than B, it is the first of these two ways which takes place [*i.e.* determination changes, *e.g.* rule 4], and when it is smaller, it is the second [*i.e.* the resistance is overcome and motion is transferred, *e.g.* rule 5], and finally, when they are equal, this change is made half in the one way and half in the other [*e.g.* rule 6].[333]

Descartes then refers back to the statement of *size* in the numerical examples he provided in the second paragraph to elucidate the principle. Since he had restated size in terms of the amount of motion that could be transferred (up to half of speed of the moving body), Descartes now uses that language to illustrate the PLMC.

> For when C is the largest, B could push it ahead of itself only if it were to transfer to C more than half its speed, and at the same time more than half of its determination to go from right to left, insofar as this determination is joined to its speed. So instead, being reflected, without changing C, it changes only its entire determination, which is a lesser change than a change of more than half of this same determination and more than half of the speed.

Descartes has established a set of equivalences. As a principle, a smaller body cannot move a larger body at rest. If it could move the larger body, it would transfer enough motion that they would move together (pushing the larger in front of itself). This would require transferring more than half of its motion to the body at rest. But smaller bodies cannot move larger bodies at rest. Consequently bodies cannot transfer more than half of their motion to bodies at rest. As we have seen in paragraph 2, these are equivalent

[333] Ibid.

statements. Another way to say this same thing is the following: a smaller body C changes its entire determination (it reflects) rather than transfer more than half its motion and accompanying determination of that motion. In paragraphs three and four, Descartes is calling Clerselier's attention to the fact that this same principle that he has been describing throughout the letter can also be described as the "least modal change." A change in the entire determination of C is smaller than a transfer of "more than half of its speed" in addition to the accompanying determination of this speed.

Descartes again contrasts the scenario in rule 4 with that in rule 5, using the same set of equivalences:

> On the other hand, if C were smaller than B, it should be pushed by it, for then B give it less than half of its own speed and less than half of the determination which is joined to it. This is less than changing the entire determination, which would be changed if it were reflected.[334]

Descartes has already shown that when a larger body transfers motion to a smaller body at rest such that they both move together after impact, the larger transfers less than half of its motion. Another description of the same principle is that a transfer of less than half of the moving body's motion (together with the accompanying determination) is less than a change in the entire determination.

The explanation of the PLMC is closely connected to the impact law. It is an alternative statement of the same fundamental idea that *size* is the relevant condition for rebound. The PLMC is also closely connected to the previous paragraphs of the letter to Clerselier. The explanation of the PLMC relies on the notion from the second paragraph that a moving body can transfer up to half of its speed to a body at rest. This condition is

[334] Ibid.

not arbitrary. It is merely a restatement of the condition that a smaller body does not transfer motion to a larger body at rest.

Section 6
Conclusion

Descartes's view of collision changed and developed. In his early view, he rejected that which would become his notorious fourth rule of collision, which states that a smaller body cannot move a larger body at rest. In the year prior to writing the *Principles of Philosophy,* Descartes describes smaller bodies moving larger bodies at rest in every context in which he describes collision—whether the bodies are in a medium, whether they are illustrating another principle such as the conservation of quantity of motion, or most importantly, whether they are quantitatively described in the abstract, isolated from all other bodies. In every case, the smaller body transfers as much of its motion such that both bodies move together after meeting. Throughout his early view, he used the same inverse proportion that Beeckman had used: the speed of the bodies after collision is to the speed of the moving body before, as the size of the initially moving body is to the size of both bodies moving together after. In Descartes's later view, as found in the Latin and French editions of the *Principles of Philosophy* and the letter to Clerselier, he changed his account of collision to include rule four.

Descartes's impact law remains largely the same[335] throughout the development. The significant change between his early and later view is that in the latter he stipulates

[335] There are small exceptions. The order in which the rule is found changes from *The World*, where it is the second law of nature, to the *Principles of Philosophy*, where it is the third law of nature. The conditions of the impact law, transfer of motion and rebound, are more clearly expressed in the later work.

the conditions of the impact law—specifically, he articulates when the force of resistance is *not* overcome in the contest between moving force and force of resistance. Although his impact law had always claimed that a failure to overcome the force of resistance results in rebound with no transfer of motion, it was not until the rules of collision in the *Principles of Philosophy* that Descartes specified when this would occur, *i.e.* when the force of resistance is actually greater than the moving force. In every quantitative example that Descartes presented in his early view, the moving force overcomes the force of resistance, *e.g.* a falling stone overcomes the resistance of the entire earth. Rule 4 stipulates the condition in which the force of resistance is greater than the moving force. Contrary to the standard interpretation, I have argued that the relevant condition is the *size* of the body.

New light is shed on Descartes's rules of collision if we focus our attention on this shift, and the corresponding importance of the outcomes of collision—whether the force of resistance is greater resulting in rebound, or whether the moving force is greater resulting in a transfer of motion, or whether the force of resistance and moving force are equal. With this focus, together with attention to the historicity of the mathematics used by Descartes himself, rather than a *reconstruction* of Descartes's ideas with modern mathematics, I have uncovered a unifying pattern throughout the rules. There is a distinct similarity in the structure of those rules that share an outcome of the impact law—the force of resistance is not overcome resulting in rebound such as rules 4, 2, and 7b, the force of resistance is overcome resulting in a transfer of motion such as rules 5, 3, 7a, or there is a balance between the forces such as 6, 7c, 1.

All of the rules abide by several prior principles: the conservation of quantity of motion, the contest of forces described by the impact law, and the condition in which the force of resistance is greater than the moving force. While explaining his rules, such as rule 4, Descartes presents the various possible outcomes. He describes what would occur if the force of resistance were overcome and motion were to be transferred, just as he describes what would happen if it was not and there was rebound instead. From the possible outcomes he works "backward." If a contradiction with a prior principle was found, the possible outcome was eliminated. Descartes was explicit while explaining rule 4 to describe how much motion would be transferred, if the force of resistance were to be overcome. But it is not possible to overcome the force of resistance in the scenario described by rule 4. Similarly in rule 6 (in which the bodies are equal and one is at rest), Descartes presents the possible outcomes—rebound or transfer of motion. However, the force of resistance and moving force are equal. Neither option can be eliminated, so he evenly divided the amount of motion that would either be transferred or retained, and both rebound and transfer occurs. Precisely the same reasoning is used in rule 7c. He uses a similar explanatory strategy in rule 1. However, here one option must be eliminated, not because it conflicts with the condition of rebound, but rather because it conflicts with another prior principle, the conservation of quantity of motion. The rules of collision were a new problem for Descartes, and Descartes used a problem-solving strategy—the analytic method—to determine his rules.

The standard interpretation of Descartes's rules of collision has presented the quantity of motion as the product of body and speed. Descartes could have easily expressed the quantity of motion in this manner, but he never did. The forces involved in the impact law, such as the moving force and the force of resistance, have likewise been expressed as algebraic products akin to the quantity of motion. Not only did Descartes not express these forces as products, three of Descartes's rules describe one body at rest. The above interpretation would imply that the force of resistance in the body at rest should be nil. This is an interpretive difficulty since the force of resistance in the body at rest is greater than the moving force in rule 4, and is equal to the moving force in rule 6. The solution to this difficulty has been to continue to express the force of resistance of a product, but rather than the product of the body and the speed of the body at rest, it has been argued to be the product of the body and the speed of the body approaching. I have argued that this interpretation is incorrect. I have also argued such an interpretation has led to several unnecessary interpretive problems with both Descartes's explanations in the letter to Clerselier as well as the French edition of the *Principles*. Using the received view of the force of resistance as a lens, it has been argued that there is a proliferation of new principles in the letter to Clerselier, with some, such as the Principle of Least Modal Change, bearing no connection to the impact law, and others seeming to be completely arbitrary with no justification at all. The subsequent French edition of the *Principles* relies on a hybrid of some of these principles in the justification of the rules of collision, but is ultimately a hastily composed unfinished draft.

If we maintain a historicist focus on the mathematics that Descartes himself used, and if we are attentive to Descartes's own guidance for understanding the rules of collision according to the outcomes of the contest between the force of resistance and moving force, we find that Descartes's rules of collision are much less problematic than has usually been alleged. There is an underlying pattern and *method* unifying the rules of collision, which we see clearly when they are reorganized as I have suggested. This chapter has attempted to make sense of Descartes's practice, and has recaptured the method that was advocated and practiced by Descartes and others in the 17th century, the analytic method. I have shown that the letter to Clerselier is a well-ordered letter, in which Descartes explains himself in three interlocking ways. Rather than a hybrid of ideas in the French edition, we find Descartes revealing the analytic method to further explain how he attempted to solve the problem of collision.

Descartes's rules of collision were pivotal. They framed the topic in published form that was only touched upon by Beeckman in private. And Descartes tackled new problems such as an account of rebound using the analytic method. Descartes did not, however, use symbolic algebra, with which he was so familiar from his work on analytic geometry. As we will see in the next chapter, young Christiaan Huygens would use these very tools to challenge Descartes's rules of collision.

Chapter 4
The formulation of Huygens's rules of collision: Challenging Descartes with Cartesian tools

Mr. Descartes had found the way to have his conjectures and fictions taken for truths. And to those who read his Principles of Philosophy something happened like that which happens to those who read novels which please and make the same impression as true stories. The novelty of the images of his little particles and vortices are most agreeable. When I read the book of Principles the first time, it seemed to me that everything proceeded perfectly; and when I found some difficulty, I believed it was my fault in not fully understanding his thought. I was only fifteen or sixteen years old. But since then, having discovered in it from time to time things that are obviously false and others that are very improbable, I have rid myself entirely of the prepossession I had conceived and I now find almost nothing in all his physics that I can accept as true, nor in his metaphysics and his meteorology.[336]

Mr. Descartes, who seemed to me to be jealous of the fame of Galileo, had a great desire to be regarded as the author of a new philosophy. It appears from his hopes and efforts that he wished to have it taught in the academies in place of Aristotle so that he wished that the Jesuits would embrace it; but in the pursuit of this goal, he maintained positions inconsistently which he had at one time brought forward, although they were often very false. ... He was assured of certain things without demonstration, such as the laws of motion in collision which he thought to make accepted as true in permitting it to be believed that his entire physics would be false if his laws were. It is almost as though he wished to prove them by taking an oath. However, there is only one of his laws which is true and it will be easy for me to prove it.[337]

—Christiaan Huygens's 1693 annotations on Baillet's *La vie de monsieur Des-Cartes* (1691)

[336] HOC 10: 403. Translated by Richard Westfall, *Force in Newton's Physics: The Science of Dynamics in the Seventeenth Century* (New York: American Elsevier, 1971) 185.
[337] HOC 10: 405. Translation by Robert S. Westman, "Huygens and the problem of Cartesianism," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980) 98.

CHAPTER 4 OUTLINE

Section 1
Introduction

In April of 1661, Christiaan Huygens met with Christopher Wren, Lawrence Rooke, John Wallis, and other members of the newly formed Royal Society in London to discuss problems concerning impact. Together they observed experiments such as the collision of two equal-length pendulums. When a one-pound bob was raised 40 degrees from the equilibrium position, for example, they were to predict what would happen after it hit a half-pound bob at rest.[338] While at the meeting, Huygens quickly made a few calculations and correctly predicted this and several other related problems that the president of the Royal Society, William Brouncker, had proposed.[339] No one else, including Wren and Rooke, could produce satisfactory rules for accurate prediction. At the time Huygens only reported his findings, preferring to blot out his calculations and kept his rules of impact to himself. In Sir Robert Moray's recollection of the event, Moray explicitly described the process that Huygens used as an "algebraicall computation," and noted that he was so successful that "every body wondered & concluded after 2 or 3 such tryalls, his rules were good."[340]

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[338] HOC 5: 547. H. Oldenburg to B. de Spinoza, 18 December 1665. The particulars of this experiment have been recorded in a letter from Henry Oldenburg, the Royal Society's Secretary and the first editor of the *Philosophical Transactions*, to Baruch Spinoza. "*Praesens non sui, quando Dominus Hugenius Experimenta, Hypothesin suam comprobantia, hic Londini fecit. Intelligo interim, quendam inter alia pilam unius librae, penduli in modum suspendisse, quae delapsa percusserit aliam, eodem modo suspensam, sed librae dimidiae, ex angulo quadraginta graduum, et Hugenium praedixisse, paucula facta Computatione Algebraica, quis foret effectus, et hunc ipsum praedictioni ad amussim respondisse.*" Also see Domenico Bertoloni Meli, *Thinking with Objects* (Baltimore: The Johns Hopkins University Press, 2006), 233-4.

[339] HOC 16: 172-3. Diederik Korteweg, the editor of this portion of volume 16, specifically notes that Huygens used "un petit calcul algèbrique."

[340] OCH 2: 624-5. Moray to Oldenburg, 27 November 1665. In the letter Moray provides a similar description of the experiment as that of Oldenburg, noted above: "Dr Wren had made the Experiments & onely stated the questions to Mr Hugens, that hee had tryed as for Example a ball, of one pound weight hung lyke a pendulum, let fall so as to strike another iust so hung, from an Angle of 40. degrees the other being half so heavy what should be the effect? Mr Hugens in about a minutes time having made a little Algebraicall computation solved the Question so iust that every body wondered & concluded after 2. or 3 such tryalls, his rules were good."

A number of Huygens's early works use algebraic techniques that are presumably of the same type as those he used in London. Key manuscripts from the early 1650s contain many of Huygens's early thoughts on collision. These would later be refined and expressed in his draft *De motu corporum ex percussione.*[341] On the documents from the early 50s, however, and notably not on the later draft of *De motu corporum ex percussione*, are algebraic expressions of collision. The symbolic algebra contained in this document may well be among the first equations to represent *physical relations* in the history of science.[342] Their significance is that different kinds of quantity, such as the speed of bodies and the size of bodies are included in a single equation. Previously algebra had been used to study curves and space; it had also been used to investigate a single kind of physical quantity in a given equation. Huygens's equations contain relations of different kinds of physical quantities. Despite their deceptive simplicity, these equations marked a change in the way mathematics was used to study nature and opened up new avenues of research in mechanics. Huygens clearly relied on Cartesian

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[341] HOC 16: 30-91. Alan Gabbey, "Huygens and mechanics," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980) 168-70. *De motu corporum ex percussione* was written by 1656, but was not published until 1703, after Huygens's death. This does not mean that his theory of collision was unknown. He had shared several theorems with his correspondents between 1656 and 1661, defended his collision theory at the Académie des Sciences in early 1668, published 7 propositions (without proofs) with the *Journal des Sçavans*, and published a Latin version of the propositions with the Royal Society's *Philosophical Transactions* in April 1669, which was meant to appear with the contributions of Wren and Wallis in the previous issue of the *Philosophical Transactions*.

[342] C. D. Andriesse, *Huygens: The Man Behind the Principle* (New York: Cambridge University Press, 2005), 103. Bertoloni Meli, *Thinking with Objects*, 232. John Roche, *Mathematics of Measurement: A Critical History* (London: Athlone Press, 1998), 90-1, 106. Andriesse states clearly that these might have been the first physical formulas ever to be written. Bertoloni Meli does not mention that they were the first equations. He does, however, remark upon the new algebraic methods of solving equations, notes the use of the now familiar conventional symbols *x* and *y*, and claims that along with experiments involving the pendulum and other devices, Huygens's algebraic equations were part of the "heuristic path" leading to the "masterful axiomatic theory" found in *De motu corporum ex percussione*. Roche claims that Huygens's 1652 manuscript was the first to represent quantity of motion as the algebraic product of mass and velocity. He presents several examples of early algebraic representations of physical phenomena, *e.g.* Tartaglia's verbal algebra in 1546 to describe a balance, John Dee's symbolic algebra to calculate intermediate temperatures in 1570, and Marinus Ghetaldi's 1630 posthumous publication in which algebra determines the amounts of gold and silver in a crown.

mathematical tools, which he had learned from his teacher, Frans van Schooten, but he developed them in novel ways. In fact, Huygens's well-known corrections of Descartes's physics—the rules of collision as well as the conservation of the quantity of motion—are presented in this early document. In other words, in 1652 Huygens challenged Descartes's physics with Cartesian mathematical weapons.

In addition to a critical conceptual analysis, Huygens's algebraic equations were likely significant in the scaffolding of a new principle, the conservation of Cartesian quantity of motion with direction, which combines subtraction with the direction of motion. Corresponding to Huygens's algebraic investigations of collision are his influential studies of the pendulum. The pendulum proved to be important to collision, not only in Huygens empirical studies, but also for the formation of several of his fundamental principles, such as his variation of the Torricelli principle, the reversibility of impact, and the conservation of body times speed squared.

With Huygens, we again see the importance of understanding the historicity of the mathematics of collision. Although the algebra in Huygens's manuscripts was innovative, it is not identical to modern symbolic equations. It holds, rather, a status "in-between" the flexibility of a formal system and the traditional requirements on quantities. Likely through his struggles with negative quantities, which he sought to avoid in his algebraic equations for collision, Huygens developed the new principle of conservation of Cartesian quantity of motion with direction. Although Huygens's algebraic calculations, which accurately predicted the motions of colliding pendulum bobs, were influential (Wren and Wallis would publish their own studies of collision with algebra), Huygens's did not publish them. Rather, in the works prepared for publication, Huygens formulated his theory in the axiomatic tradition of Archimedes and Galileo, using geometric diagrams to represent physical quantities as continuous magnitudes, rigorously employing the theory of proportions. Here his axioms relied on principles of symmetry.

Huygens was deeply influenced by Descartes's theory of collision and mathematics, and shared Descartes's vision that all natural phenomena can be explained by the motion of particles. However, he challenged the Cartesian rules of collision at the foundation of this vision, using Descartes's symbolic algebra. With algebra as a heuristic and the pendulum as a key object in the formation of his principles, Huygens presented and justified his theory in an axiomatic formulation. Using Descartes's first rule, which describes a symmetrical collision, and the relativity of motion as a principle of invariance, he established his theory of collision and challenged Descartes, re-conceptualizing the fundamental concepts of matter and motion in the process.

Section 2
Algebraic collisions

Huygens learned Cartesian algebra from an early age. He became adept and used the new mathematics in novel ways, expressing physical quantities relating to collision in equations, such as the conservation of Cartesian quantity of motion and Huygens's principle of the conservation of "body times speed squared." He used the equations to analyze Descartes's rules of collision, specifically rules 4 and 5. Huygens's algebraic equations of collision hold an in-between-status between the flexibility of modern algebra, and traditional requirements on quantity. They are not indicative of a formal system; the symbols refer to specific quantities, and are bound by the restraints such quantities impose. For example, Huygens takes steps to avoid the “negative quantities” that are almost inevitably produced when calculating the acquired speed (after impact) of an initially moving body. But the equations are not representations of geometrical objects (such as steps of a construction as was the case with Harriot in chapter 2, or of curves as in Descartes's analytic geometry), nor do they adhere to the rules on the relations of kinds of quantities in the theory of proportions (as does the mathematics in Huygens's published *Horologium oscillatorium*). In addition to its predictive role in calculating the experimental outcomes of colliding pendulum bobs, and the heuristic role in clarifying Huygens's position against Descartes, the algebra may have also been instrumental in the formation of Huygens’s principle of the conservation of (“Cartesian”) quantity of motion with direction.

2.1 – Education in Cartesian Mathematics

Descartes had recommended Frans van Schooten (1615-1660) to Christiaan’s father, Constantijn, as a tutor for his children, writing that “perhaps you [Constantijn] could quite easily afford yourself the opportunity to introduce Algebra to any of your children who might have some inclination in that direction; for I know of no one in this country and I know scarcely anyone anywhere who is as capable as he [Van Schooten] is.”[343] Van Schooten did give private lessons to Christiaan and his brother,[344] and taught mathematics at Leiden University while Christiaan studied law and mathematics there

[343] HOC 22: 50. Descartes to Constantijn Huygens, 21 December 1644. Translation by Westman, "Huygens and Cartesianism," 86. The letter is also collected in Leon Roth, ed., *Correspondence of Descartes and Constantyn Huygens, 1635-1647* (New York: Oxford University Press, 1926), 234-5. Also see Nicole Christine Howard, "Christiaan Huygens: The Construction of Texts and Audiences" (PhD diss., Indiana University, 2003), 38.
[344] J. A. van Maanen, *Facets of Seventeenth Century Mathematics in the Netherlands* (Utrecht: Drukkerij Elinkwijk B. V., 1987), 21.

from 1645-1647.[345] At this time Van Schooten had taught the teenage Huygens Cartesian analytic geometry.[346] The algebraic-analytic method is apparent, for example, in Huygens's "homework" papers on a series of geometrical problems as early as 1645.[347] In addition to advocating Cartesian natural philosophy and promoting Descartes's analytical mathematical methods to his students and an international audience, Van Schooten was also involved with "Duytsche mathematique" at the engineering school affiliated with Leiden University.[348] The school, established in 1600, taught practical mathematics[349] in the vernacular. Frans van Schooten's father was chair of "Dutch mathematics" starting in 1615, and was succeeded by his son, and later Frans's brother Petrus. When Petrus died, the school closed. Huygens's later works, particularly those on lenses and pendulum clocks, display a unique balance of the practical and abstract, which may have been cultivated at least in part from Van Schooten's instruction.[350]

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[345] H. J. M. Bos, "Huygens and mathematics," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980) 126-46. Bos has conceptually divided Huygens's mathematical career into several periods: 1645-1655 is his "formative period;" 1655-1660 is his "creative period;" 1660-1680 is a period dominated primarily by transcendental curves and inverse calculus problems.

[346] Andriesse, *Huygens*, 73. H. J. M. Bos, "Christiaan Huygens: a biographical sketch," *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980). Dirk Struik, *The Land of Stevin and Huygens* (Boston: Reidel, 1981), 90. Westman, "Huygens and Cartesianism," 86.

[347] HOC 1: 5-10. Stampioen de Jonge to [Christiaan Huygens], [1645]. Stampioen de Jonge was Christiaan's teacher at Leiden prior to van Schooten. The letter includes a long list of mathematical works that Huygens was to study. The first title on the list is *Hondert Geometrische questien met hare solutien* by Sybrandt Hansz Cardinael. Huygens produced solutions to these problems using algebraic techniques. They date to the time that Frans van Schooten replaced de Jonge as Huygens's teacher. See HOC 11: 23-7.

[348] Struik, *Land of Stevin and Huygens*, 85. Maanen, *Facets*, 5-8.

[349] Maanen, *Facets*, 8. The school would have attracted professionals such as surveyors, navigators, astronomers, architects, designers of fortification, teachers of arithmetic ("rekenmeesters"), university professors of mathematics, and compilers of mathematical tables.

[350] Maanen, *Facets*, 18-20. There were significant economic changes in the Netherlands around the midcentury, namely the level of affluence substantially increased. Van Maanen has argued that with this economic change came a new category of student who came from a more wealthy background. Van Maanen mentions Huygens among his examples. The new economic growth also brought a shift in emphasis in teaching, and subsequently in research, to "non-practical" fields of study. Mathematics particularly benefited from this shift. There is a correlated increase in the number of students and the number of institutions for higher education with the economic changes. Van Maanen mentions that Frans van Schooten profited from these circumstances and gathered several young mathematicians around him and stimulated interest in Descartes's *La Géométrie*. Huygens's later pursuits and publications demonstrate

Huygens not only studied and learned the new Cartesian mathematical methods from Van Schooten, but also quickly became proficient enough to contribute to both the first[351] and second[352] editions of Van Schooten's Latin translation and systematization of Descartes's *La Géométrie*.[353]

Huygens continued to apply these new methods to other areas of his study of nature. Among the first of these are the equations at the bottom of a large manuscript sheet[354] dating to 1652,[355] and his notes and examples of particular collisions from 1654.[356] See figures 1 and 2. These manuscripts are important because (1) they are among his earliest writings on collision, and (2) the writing of the 1652 document corresponds to Huygens's first announcements of his criticism of Descartes.

The 1652 document may have begun as a draft of a letter to Van Schooten[357] in which Huygens requested a book on plane loci and discussed an exchange with Gregory of St. Vincent, whose quadrature of the circle Huygens had recently refuted in his first

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that he was not exclusively interested in abstract problems. Perhaps Van Schooten's "Duytsche Mathematique" was an influence along with *La Géométrie*.

[351] HOC 14: 410-5, 416-7.

[352] HOC 14: 417-27.

[353] Maanen, *Facets*, 20-1.

[354] HOC 16: 6-10. Korteweg, the editor of volume 16, provides a general description of this manuscript in his *avertissement*.

[355] HOC 16: 94-9.

[356] HOC 16: 132-6. Some time later in Huygens's life, possibly when he was arranging his papers to be given to a library, he had collected and numbered the 1652 manuscript sheet together with those dating to 1654. These are compiled as *Appendice I* to *De motu corporum ex percussione* in HOC 16. The editors have divided *Appendice I* into eleven parts, which ideally correspond to the themes and pagination of Huygens's work and numbering system. Parts two and three, for example, correspond to the 1652 manuscript (HOC 16: 94-9). However, as Joella Yoder has indicated, although Huygens had willed his manuscripts directly to a library, their history is quite complex. His original arrangement, particularly of loose manuscripts, has been all but obliterated. Thus, this portion of Huygens's manuscripts is fragmentary and disorderly which makes my attempts to analyze and draw inferences from them especially challenging. See Joella Yoder, "The Archive of Christiaan Huygens and his Editors," in *Archives of the Scientific Revolution*, ed. Michael Hunter (Woodbridge: Boydell, 1998), 91-108.

[357] HOC 16: 6-7. Korteweg identifies the manuscript as a draft of a letter to which Van Schooten responded on 28 July 1652. See HOC 1: 183 for Van Schooten's response.

publication, *Theoremata de quadratura hyperboles, ellipsis et circuli* (1651).[358] But the manuscript came to be used as a penetrating investigation into the collision of bodies, and no shred of the paper was wasted: it has been entirely filed with symbols, diagrams, and propositions, with script running in at least three different directions. Some propositions have been struck out with bold lines, others circled repeatedly, and the numbering of several axioms have been scribbled out and rewritten. Diagrams are drawn in almost every available space in many orientations, and most look like they refer to collisions. Several appear to represent pendulums, and one may refer to a collision occurring on a circular track. The pictures of pendulums are particularly significant, given Huygens's longstanding interest in their principles, and because they were beginning to be used as experimental devices for collision. The height of the bob could be used to measure speed—thus, colliding pendulums provided access to the speeds of bodies before and after impact. Algebraic equations are written on the "bottom" of the manuscript (depending on which way the page is oriented). See figure 1.

[358] HOC 16: 99.

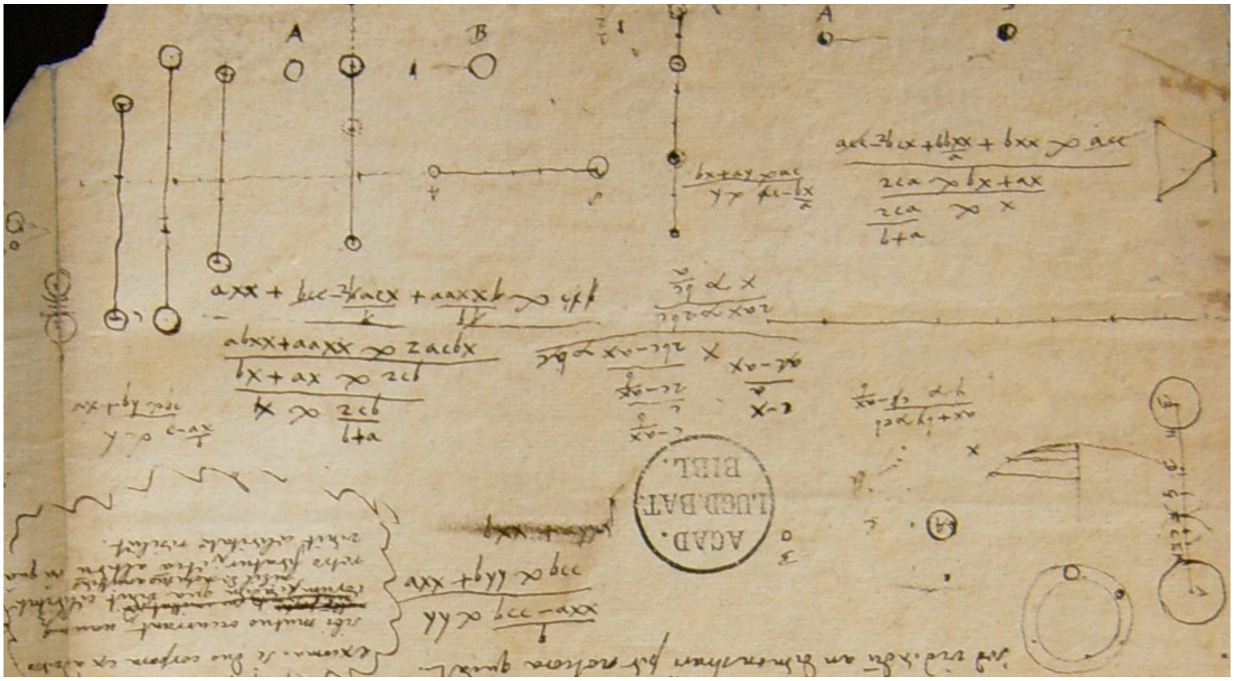

Figure 1. Equations from the 1652 manuscript. The manuscript can be found in the *Codices Hugeniorum 26A*, fol. 9r, henceforth abbreviated as HUG 26 fol. 9r. The image has been cropped to include only the lower half of the page.

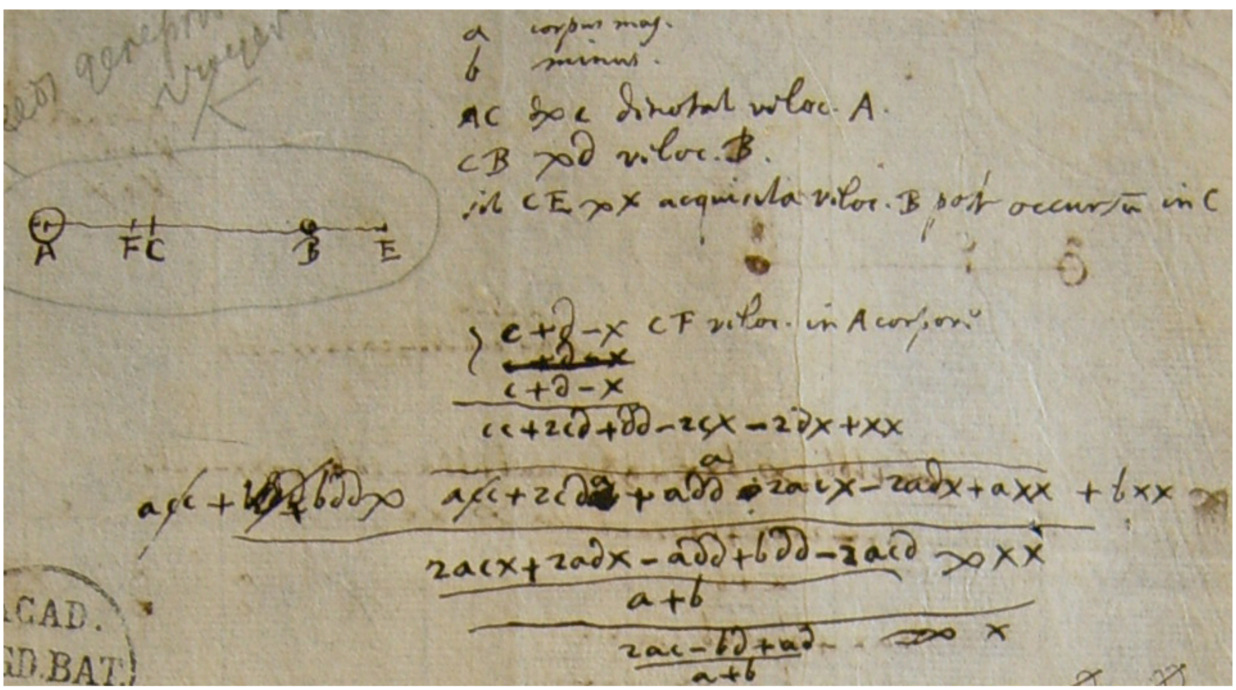

Figure 2. Naming convention and equations from the 1654 notes. The image, which also comes from HUG26A, is cropped from the inside r^{o} of a large folded manuscript, which has been labeled 26 in pencil in the upper left corner and 33 in ink in the upper right corner, presumably by Huygens. The image is cropped to the upper half of this page.

2.2 – Physical Equations: algebraic expressions of Cartesian collisions

The top most set of equations[359] on the 1652 manuscript can be found in figure 3. The equations on the left of figure 3 are listed as 1 – 2. The equations on the right of figure 3 are listed as 3 – 5.

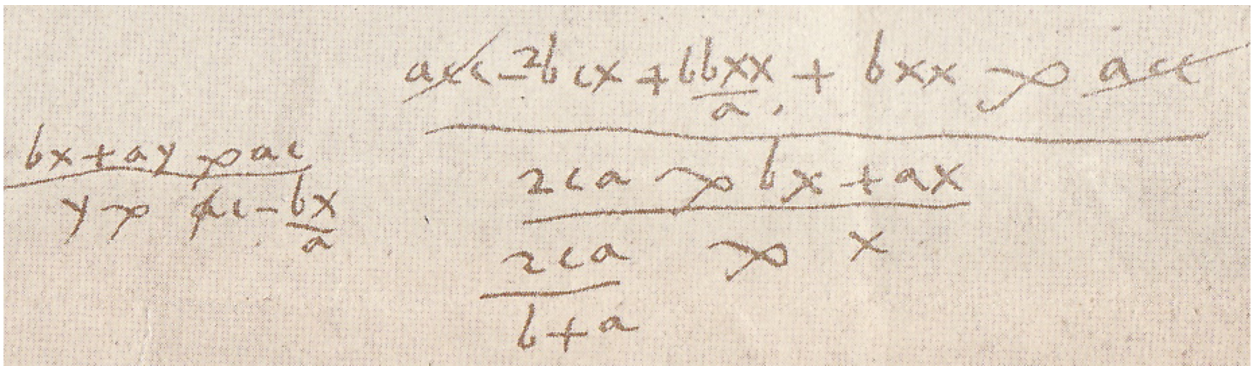

Figure 3

$$bx + ay = ac \qquad (1)$$

$$y = c - bx / a \qquad (2)$$

$$acc - 2bcx + bbxx/a + bxx = acc \qquad (3)$$

$$2ca = bx + ax \qquad (4)$$

$$2ca/(b+a) = x \qquad (5)$$

To the left and down from these equations, on the original manuscript (as can be seen in figure 1), is another similar set of equations.[360] See figure 4. The equations on the left of figure 4 are listed as 6 – 7. The equations on the right of figure 4 are listed as 8 – 11.

[359] HOC 16: 98. The equations, as written on the manuscript, use a different sign for equality than the modern and familiar "=." The significance of the original equality sign is remarked upon in a footnote below. Departing from the manuscript, I have used the modern sign "=." There are also several marks on the manuscript, which indicate simple algebraic simplifications, such as the crossed *a* in what would be equation (2) and the crossed *acc* in what would be equation (3). For ease of reading and formatting, I have presented the equations without these marks in the text below. All of this can, however, be found in the figures, which are reproduced from the original manuscripts.
[360] HOC 16: 98.

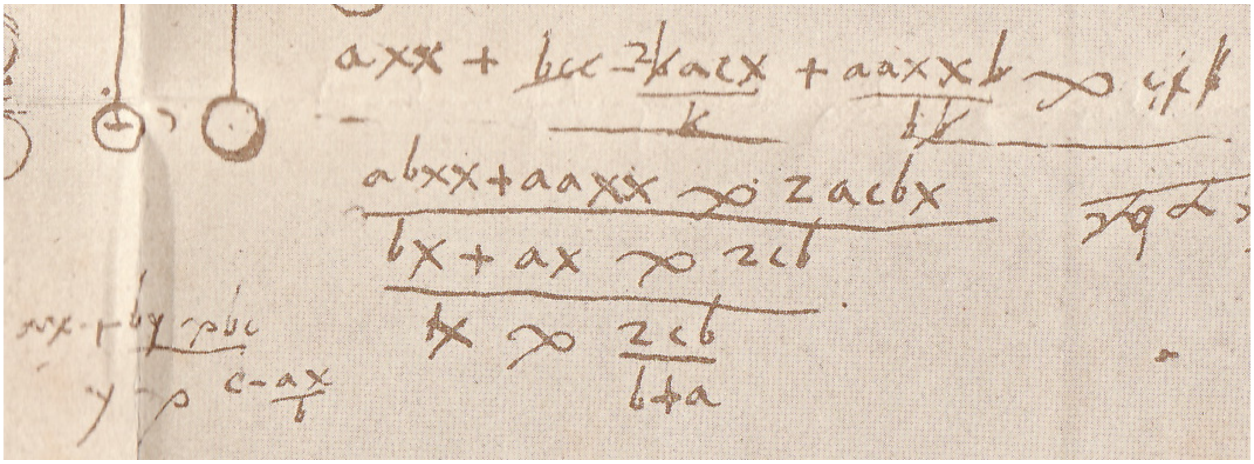

Figure 4

$$ax + by = bc \qquad (6)$$

$$y = c - ax/b \qquad (7)$$

$$axx + bcc - 2bacx/b + aaxxb/bb = ccb \qquad (8)$$

$$abxx + aaxx = 2acbx \qquad (9)$$

$$bx + ax = 2cb \qquad (10)$$

$$x = 2cb/(b+a) \qquad (11)$$

Notice that the left equations are simpler, first-order equations. The right, quadratic equations, are more largely and boldly written. The equations on the left solve for *y*, whereas the equations on the right solve for *x*. Note too that those solved for *y* (eq. 2 and 7) are in terms of *x*, while those solved for *x* (eq. 5 and 11) are *not* in terms of *y*. A third set of equations[361] was written below and to the right of the previous set. See figure 5. The equations in figure 5 are listed as 12 – 13.

[361] HOC 16. 98.

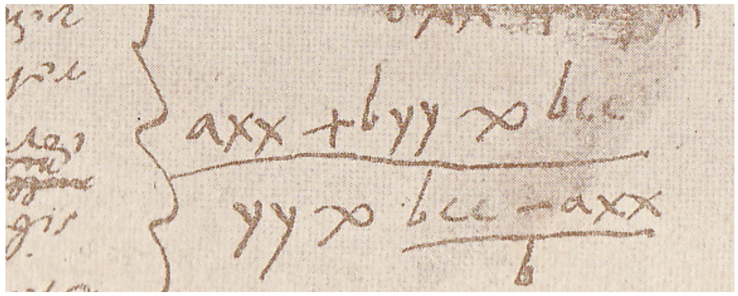

Figure 5

$$axx + byy = bcc \qquad (12)$$

$$yy = (bcc - axx)/b \qquad (13)$$

The doubled letters refer to multiplication. So, xx is $x \cdot x$, or x^2. This and other notation conventions (*e.g.* letters from the end of the alphabet for unknowns, letters from the beginning of the alphabet for known constants, and the equality symbol) are in the tradition of Descartes.[362] So too is the "key" that Huygens provided to his algebraic notation:[363]

> *a* corpus maj.
> *b* minus.
> *AC* = *c* denotat veloc. *A*.
> *CB* = *d* veloc. *B*.
> Sit *CE* = *x* acquisita veloc. *B* post occursum in *C*

Lines *AC*, *CB*, and *CE* refer to a diagram, which depicts the motion of bodies in collision.

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[362] Florian Cajori, *A History of Mathematical Notations* (La Salle: Open Court, 1928), 302. Huygens did not se the "=" sign to represent equality. Instead he adopted Descartes's sign, which when typed resembles a backward "e" connected to an "o." According to Cajori, "Descartes' symbol for equality, as it appears in his *Géométrie* of 1637, is simply the astronomical symbol for Taurus, placed sideways, with the opening turned to the left... [A]s Descartes lived in Holland several years...it is not surprising that Dutch writers should be the first to adopt widely the new notation. ... [I]nfluential was Christiaan Huygens who used [the symbol] as early as 1646 and in his subsequent writings. In the 1652 manuscript, of course, the symbol, which resembles the "proportionality sign," had been written rather than set in type.

[363] HOC 16: 132-6.

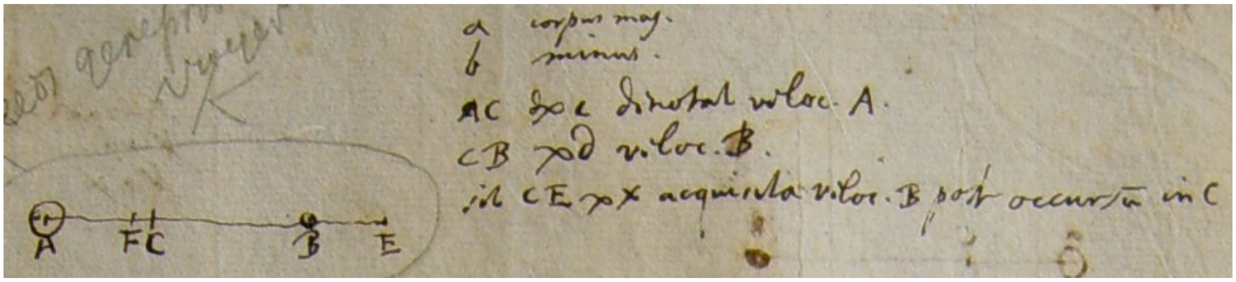

Figure 6. Diagram and key from the 1654 notes

Huygens consistently used this type of diagram—a horizontal line, whose segments represent speeds, with circles near each end of the line representing the bodies involved in collision—in his first works on collision from the early 1650s through his completed *De motu corporum ex percussione*. The above key is strikingly similar to what Descartes prescribed in the opening pages of *La Géométrie:*

> So that we may be sure to remember the names of these lines, a separate list should always be made as often as names are assigned or changed. For example, we may write,
>
> AB = 1, that is AB is equal to 1;
> $GH = a$,
> $BD = b$, and so on.[364]

In Huygens's early work he relied on, as Descartes did, three methods of representation—the diagram, the language of geometry, and the algebraic symbol. The relationship between the diagrams and equations is quite complex. The physical situation of bodies and speeds is represented through the diagram. Using algebra, the relations among the physical quantities are subsequently analyzed. This marks an important distinction with both Descartes, whose algebra (in *La Géométrie*) was used to solve classical problems in geometry, with equations referring to lines and curves, and Harriot, who used symbolic equations in his *De reflexione corporum rotundorum* to represent the steps of the construction of a geometric diagram. Huygens represents bodies (which for

[364] AT VI 372. *La Géométrie* I. Translation by David Eugene Smith and Marcia L. Latham, *The Geometry of René Descartes with a facsimile of the first edition* (New York: Dover, 1954), 8.

Huygens were not identical to extension, as they were for Descartes) and, as we shall see, speed with direction. Although Huygens used diagrams throughout his study of collision, in his first works on collision from 1652, which were not intended for publication, algebra plays an important role.

Equation 1 is an algebraic expression of the conservation of Cartesian quantity of motion. Here is why—Huygens's Cartesian symbols depicted on the left in figure 3 (eq. 1 and 2) are defined as follows:

a = body[365] A	c =speed of A before impact	y =speed of A after impact
b =body B		x =speed of B after impact

Understandably, the speeds after impact are depicted as "unknowns." And the speed before impact is a "known quantity." Equation 1, $bx + ay = ac$, describes the following scenario. There are two bodies A and B. A is moving with speed c, and B is at rest. They collide. After impact, body B moves with speed x, while body A moves with speed y. The "Cartesian quantity of motion," (body times scalar speed) on the left side of the equation is equal to that on the right; in other words, it is the same before and after impact. The quantity of motion is "conserved."

The second set of equations, shown in figure 4, is nearly identical to the first set (found in figure 3). Equation 6 from the second set ($ax+by=bc$) and equation 1 from the first set ($bx+ay=ac$) are alike except that the *a*s and *b*s have switched places with each other. This slight change has significant implications. The symbols in this second set of equations shown in figure 4 are defined as follows:

[365] "body *A*" refers to the "bulk," "weight," or "size" of *A*. This rather general expression of the quantity of an object is not identical to "mass." I have used the word "body" to be consistent with Huygens's term *corpus*.

a = body A		x =speed of A after impact
b =body B	c =speed of B before impact	y =speed of B after impact

In the first set of equations (1 – 5) found in figure 3, body A is initially in motion and B is initially at rest. In the second set of equations (6 – 11) found in figure 4, body B is initially in motion and A is initially at rest. Unlike most modern algebraic reconstructions of Huygens's work on collision,[366] the symbols for speed in Huygens's manuscripts change their referent across the sets of equations (although they remain consistent within a set of equations). They take their meaning in the context of the known value to which they are multiplied. As we will see below, this peculiarity will have more significant consequences for our interpretation of Huygens's use of these equations.

The quadratic equations in the two sets of equations (3) and (8) are also nearly identical to each other. These equations, which are notably only in terms of x (whose referents are distinct to their specific "set of equations"), can be found if the above corresponding linear equation (the expression of Cartesian conservation of quantity of motion, solved for y, *i.e.* eq. 2 and eq. 7) is squared and substituted into the value of y in the third group of equations, which are shown in figure 5, *i.e.* equations 12 and 13 above.[367]

Equation 12 ($axx+byy=bcc$) has been called *le principe de la conservation des forces vives* by the editors of the *Oeurvres complètes de Christiaan Huygens.* The *force*

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[366] HOC 16: 22-7. Andriesse, *Huygens*, 105-12. A. E. Bell, *Christian Huygens and the Development of Science in the Seventeenth Century* (London: St. Ann's Press, 1947), 111-2. Gabbey, "Huygens and mechanics," 197n.

[367] Consider the set of equations from figure 4. Solving for y, equation 6 ($ax+by=bc$) becomes equation 7 ($y=c-ax/b$). Both sides of the equations are squared: $yy=(c-ax/b)(c-ax/b)$ to obtain $yy=cc-(2acx/b)+(aaxx/bb)$. This is substituted into the conservation of "body times speed squared" equation, *i.e.* equation 12 ($axx+byy=bcc$) to obtain $axx+b(cc-(2acx/b)+(aaxx/bb)=bcc$, *which becomes:* $axx+bcc-2bacx/b+aaxxb/bb=ccb$ or equation 8. Thus, equation 8 is the conservation of "body times speed squared" expressed in terms of x, which was achieved by a substitution of y derived from the conservation of quantity of motion. This can then be solved for x.

vive, or "living force," would become a significant concept in mechanics and natural philosophy of the late 17th century. Although the quantity originates with Huygens, he did not name it "living force." The conservation of this quantity in "elastic collisions" is one of Huygens's most famous results in his developed theory of collision, specifically as proposition XI in *De motu corporum ex percussione*. In the early 1652 manuscript, it appears just after a proposition declaring that the Cartesian quantity of motion is *not* conserved. We will return to the propositions concerning this quantity to discuss its probable connection to Huygens's understanding of the pendulum.

The two sets of quadratic equations on the 1652 manuscript thus bear a close relationship to the "body times speed squared" equation. For example, equation 8 is essentially the conservation of "body times speed squared" expressed in terms of x, which was achieved by a substitution for the value of y, which was found by squaring an equation derived from the conservation of quantity of motion. Equation 8 is then solved for x to produce equation 11 ($x= 2cb/(b+a)$).[368] Note that evidence of some of the algebraic operations is still apparent in the equations. For example, the terms "*bcc*" and "*ccb*" on both the left and right side of equation 8 (as seen in figure 4) have slash marks through them indicating that they "cancel."

The algebraic expression of the new conservation principle (equation 12) links the linear Cartesian expression (equation 6) to the more elaborate quadratic equation (equation 8) and thus to the equation solved for x (equation 11). The quadratic equations

[368] The term *bcc* is cancelled on both the left and right side of equation 8 to become $axx-2acx+aaxx/b=0$, and then $axx+aaxx/b=2acx$. he used similar marks to cross out the *b*s in the numerator and denominator as those used to "cancel" *bcc* above. To further simplify and remove the remaining *b* from the denominator, Huygens multiplied both sides of the equation by *b.* This results in equation 9 ($abxx+aaxx=2acbx$). Factoring out *ax* from both sides of the equation, *ax* can be "cancelled." Thus equation 10 is obtained ($bx+ax=2cb$). Solving the equation for *x*, one obtains equation 11: $x=2cb/(b+a)$

(5 and 11) are both solved for x. And they are both solely in terms known before the collision. They differ from each other only in the position of a and b. As noted above, the referent of the symbols depend on context and are specific to each set of equations. When body *A* is initially in motion, as it is in the first set of equations (1-5) displayed in figure 3, x refers to the speed of *B* after impact. Equation 5 ($x=2ca/(b+a)$) is the speed of *B* after impact in the scenario in which *A* is initially in motion and *B* is initially at rest. When body *B* is initially in motion, as it is in the second set of equations (6-11) displayed in figure 4, x refers to the speed of *A* after impact. Equation 11 ($x= 2cb/(b+a)$) is the speed of *A* after impact in the scenario in which *B* is initially in motion and *A* is initially at rest. Note that x always refers to the *acquired speed* of the body initially at rest. See the table below.

More precisely, throughout Huygens's equations, a does not just refer to "body *A*" as distinct from "body *B*." Whenever the bodies are of different sizes, a refers to the larger of the two. His convention is consistent in the diagrams and propositions on the 1652 manuscript,[369] the algebraic notes from 1654,[370] as well as his treatise *De motu corporum ex percussione*.

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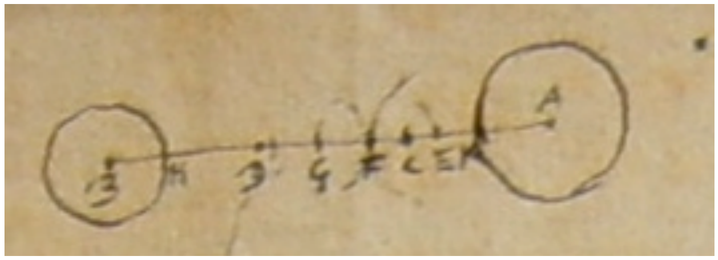
[369] Although there are several diagrams with A and B roughly the same size, there are no cases where body B is depicted greater in magnitude. The majority of the propositions written on the 1652 sheet describe a collision of unequally sized bodies. For example see HOC 16: 96. "If the larger body A should strike the smaller B, and what is more, the speed of B is the speed of A reciprocally as the magnitude A to B, then each would rebound with the same speed as each comes" *Si corpus A majus occurrat B minori, sed velocitas in B sit ad velocitatem in A reciproce ut magnitudo A ad B, tum utrumque cum eadem qua venit celeritate resiliet.* It is interesting to note that in this proposition Huygens used the theory of proportions to articulate the point. The relation, speed B : speed A :: magnitude A : magnitude B, is a comparison of two different homogeneous ratios. Compare this to proposition VIII of *De motu corporum ex percussione.* HOC 16: 53. "If two bodies whose speeds are inversely proportional to their magnitudes collide with each other, then each rebounds with the same speed which it had before the collision." Translation by Richard J. Blackwell, "Christiaan Huygens' *The Motion of Colliding Bodies*," *Isis* 68 (1977): 583. It would appear that in 1652 Huygens has mistakenly expressed this proposition rather redundantly. The relation is already stated as an inverse proportion, "speed in b : speed in a :: magnitude a : magnitude b." The word, *reciproce* would then reverse the second ratio. Not only would this be an unnecessarily convoluted way to express a



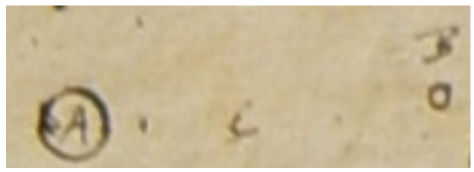


Figure 7. Diagrams of the collision of unequal bodies in the 1652 manuscript

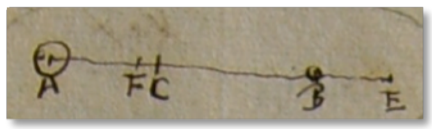


Figure 8. Diagram of collision of unequal bodies in the 1654 notes. See the key in figure 6.

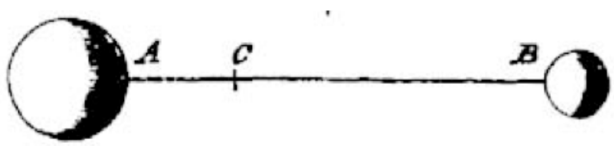


Figure 9. Diagram of collision of unequal bodies from *De motu corporum ex percussione*

Attaching the reference of *a* and *b* to specific kinds of bodies (the larger and smaller), means that the equations are not completely abstract relations of arbitrary symbols, which can be operated upon with complete disregard for the referent of the symbols. The symbols are also not entirely dependent upon the geometric diagrams; the equations themselves are meaningful objects of study. The quantities are related by equality rather

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direct proportion, but elsewhere in Huygens's writing he has expressed this proposition more clearly. In fact, this proposition functions as evidence for Huygens in his letter to Van Schooten on 29 October 1652 in which he reports that he is certain that Descartes' rules are in error. See HOC 1: 185. It is also noteworthy that the literal translation of the preposition used in this proposition is "in," *Velocitas in B sit ad velocitatem in A*. Although subtle, the difference between "in" and "of" may not be insignificant, particularly in light of the different kinds of quantity.

[370] HOC 16: 132. Huygens's exclusive use of *A* for the larger body is also corroborated in the 1654 manuscripts, mentioned above, where he explicitly defines body *A* and body *B* as "*a corpus maj.*" and "*b minus.*"

than proportion, and numerical quotients have taken the place of homogeneous relations of quantities in ratios.

This is a significant contrast with Huygens's *finished works*. Widely regarded as a brilliant geometer, Huygens strictly adhered to classical approaches to relate physical quantities in his publications.[371] For example, the *Horologium oscillatorium* (1673), often called a "geometrical physics," is organized in a strict axiomatic style and employs the theory of proportions with such rigor that, with almost no exceptions, no multiplication of dimensionally different magnitudes occurs in the whole work.[372] In this context, then, the symbols in the 1650s are surprising (1) for their very existence, but more interestingly (2) for their in-between-status. On the one hand, they do not adhere to traditional geometric rules of relations, nor are they representations of geometrical objects in the tradition of analytic geometry. On the other hand, they are not abstract *species* or symbols.[373] Huygens's equations strike a middle ground, as we shall see more clearly below.

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[371] Bell, *Huygens and the Development of Science*, 25. Bos, "Huygens and mathematics," 132. Yoder, *Unrolling Time*, 172. Many in the secondary literature have rightly commented upon his strength and preference for geometry. According to Bell, "He was hailed as the reborn Vieta and compared with Pappus and Apollonius, two giants of Greek geometry." Bos has claimed that Huygens must have *thought geometrically*. And Yoder has claimed that Huygens "regarded nature fundamentally as a geometric realm." To support her claim she cites the following: "in the *Cosmotheoros* he argues that people on other worlds would still develop Euclidean geometry because the same mathematical principles abide throughout the universe. In other words, mathematics is not an abstract construct of our earthly minds but informs nature." Yoder goes on to say that "he simply saw the physical world with the eyes of a geometer."

[372] H. J. M. Bos, "Introduction," in *Christiaan Huygens' The Pendulum Clock*, trans. Richard J. Blackwell (Ames: Iowa State university Press, 1986). Niccolò Guicciardini, *Reading the Principia* (Cambridge: Cambridge University Press, 1999), 128. Guicciardini has argued that Newton used Huygens's *Horologium oscillatorium* as a model for the *Principia*, but even so, Huygens was at times critical of Newton's non-classical use of the theory of proportions. See Guicciardini, *Reading the Principia*, 119.

[373] "Species" or "speciosæ" was a technical term used by Viète in the context of his analytic art. According to Mahoney, "unlike numerical logistic, Viète pointed out in Chapter IV of the *Introduction*, the analytic art constituted a logistic of species, an arithmetic 'set forth in terms of the species or forms of things, such as the letters of the alphabet.' The species Viète had in mind was the species of quantity [...] In the *Introduction to the Analytic Art* [...] algebra was transformed from a sophisticated sort of arithmetical problem-solving into the art of mathematical reasoning itself, insofar as that reasoning was based on combinatory operations." See Michael Mahoney, *The Mathematical Career of Pierre Fermat 1601-1665*, 2nd edition (Princeton: Princeton University Press, 1994), 36.

Given the specific physical referents of these symbols, the two duplicate sets of equations—instead of being redundant derivations—correspond to two different kinds of physical events. The symbol *a* always refers to the larger body, and the symbol *c* always refers to the speed of the body to which it is multiplied before impact. Thus, the first set of equations, displayed in figure 3, describe the event where *the larger body is initially in motion and the smaller body is initially at rest.* And the second set of equations, displayed in figure 4, describe the event where *the larger body is initially at rest and the smaller body is initially in motion*. It is not coincidental that these collision-events correspond to the initial scenarios in Descartes's rules 5 and 4. Specifically, the first set of equations (1-5) corresponds to Cartesian rule 5 and the second set of equations (6-11) corresponds to Cartesian rule 4. See the table below:

First set of equations, displayed in figure 3 Larger body *A* initially in motion, smaller body *B* initially at rest. Similar to Cartesian rule 5.			
a = body *A* b =body *B*	c =speed of *A* before impact	y =speed of *A* after impact x =speed of *B* after impact	
$bx + ay = ac$	(1)	$acc - 2bcx + bbxx/a + bxx = acc$	(3)
$y = c - bx/a$	(2)	$2ca = bx + ax$	(4)
		$2ca/(b+a) = x$	(5)

Second set of equations, displayed in figure 4 Smaller body *B* initially in motion, larger body *A* initially at rest. Similar to Cartesian rule 4.			
a = body *A* b = body *B*	 c = speed of *B* before impact	x =speed of *A* after impact y =speed of *B* after impact	
$ax + by = bc$	(6)	$axx + bcc - 2bacx/b + aaxxb/bb = ccb$	(8)
$y = c - ax/b$	(7)	$abxx + aaxx = 2acbx$	(9)
		$bx + ax = 2cb$	(10)
		$x = 2cb/(b+a)$	(11)

In a letter to Gerard van Gutschoven in January 1652, Huygens mentioned that he suspected that all but Descartes's first rule of collision were untrue, particularly singling out rule 4.[374] Over that summer, Huygens wrote up the notes that I have been calling the 1652 manuscript on what was likely a draft of a letter to Van Schooten.[375] In October of that year (1652), Huygens sent news of these first results to Van Schooten, announcing his certainty of Descartes's error.[376] The central example includes an initial scenario similar to the Cartesian rule 4. It is no surprise that his equations reflect his attention to the rules of collision. Recall that Descartes's rule 4 states:

[374] HOC 1: 166.
[375] Andriesse, *Huygens*, 104. See note above.
[376] HOC 1: 185-6. Christiaan Huygens to Fr. van Schooten, 29 October 1652. Andriesse, *Huygens*, 185.

> [I]f body C were entirely at rest, and were just a bit larger than B, then whatever the speed with which B moved toward C, it would never move C, but would be repelled by it in the opposite direction [...][377]

Huygens, of course, proposed a number of arguments against Descartes's collision rules, and rule 4 in particular, in the works he published and prepared for publication, such as the *Regulae de motu corporum ex mutuo impulsu* and *De motu corporum ex percussione*. The arguments in these axiomatic Archimedean presentations rely on symmetry and the relativity of motion, and *not* symbolic algebra. These later ideas will be discussed in section 4. Let us now consider his early algebraic investigations.

Since the equations solved for x provide the *acquired speed* of the body initially at rest in terms of quantities known prior to the collision, Huygens could easily determine, according to his equations, if the speed of the large body initially at rest will continue to be at rest after the collision. In this case, the acquired speed of the larger body initially at rest is equation 11 ($x = 2cb/(b+a)$). Thus, by Huygens's equation, rule 4 cannot be true: x, the acquired speed of the larger body initially at rest, will have a magnitude in every situation, except when either c or b is zero, but this would only occur in very trivial events.[378] This corresponds to a proposition on the 1652 manuscript, which asserts that a larger body at rest can be moved by a smaller body.[379] The algebraic mathematics that

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[377] AT VIII 68. *Principia* II 49. Translation by Daniel Garber, *Descartes' Metaphysical Physics* (Chicago: University of Chicago Press, 1992), 257.

[378] Since b is the symbol for body B, it can only be a positive magnitude. c is the initial speed of the smaller body B, and granted that speed is a continuous magnitude, it too is positive. Even if c could be negative, x would still not be zero. If both b and c are merely non-zero, then x cannot equal zero. The only way x can be zero is if c and/or b is zero. If the initial speed is c=0 then there will be no impact and the body initially at rest remains at rest. If the smaller body B has no magnitude then presumably it does not exist, and no collision occurs; the body initially at rest remains at rest. Obviously, these cases are not what Descartes had in mind.

[379] HOC 16: 95. *Majus corpus quiescens ab eodem corpore eadem celeritate impulsum minorem celeritatem acquirit quam corpus minus*. The proposition contains a stipulation that the acquired speed of the larger will be less than the initial speed of the smaller. The mathematics confirms this. If $x = 2cb/(b+a)$ and if x is the acquired speed of body A and c is the initial speed of the smaller body B, then $2cb/(b+a) > c$. This becomes $2cb < cb+ca$. Given that a is greater than b, this is true.

Huygens learned from Descartes was used to express Descartes's conservation principles (which Descartes justified by appealing to the immutability of God). This was then substituted into an equation of a new conservation principle (body times speed squared), which shows that rule 4 must be false. Although striking for the correspondence with the first announcements of Huygens's rejection of Descartes's rule 4, Huygens did not rely on this argument in his polished and published work. Nevertheless the algebra on the early manuscripts was a significant tool in the formulation of Huygens's ideas on collision.

2.3 – Colliding Interpretations: changing directions and avoiding negative quantities

The equations that are solved for x (eq. 5 and 11) determine the acquired speed of the body *initially at rest*. Algebraically determining the acquired speed of the body *initially in motion* is a more complicated matter in Huygens's manuscripts. The root of the problem resides in the physical and mathematical interpretations of the *change of direction* of the speed of a body. When the larger body is initially at rest, the value of the acquired speed of the body *initially in motion* would be negative, but Huygens appears to shun this as a legitimate quantity. Although the acquired speed of the larger body initially at rest (eq. 11) will always be positive, $x= 2cb/(b+a)$, when this value is algebraically substituted back into equation 7 ($y = c - ax/b$) to find the *acquired speed of the body initially in motion*, negative values will always be produced.[380] Complicating matters are

[380] The reason why it is impossible to have a positive value of y when the found value of x is substituted back into equation 7 ($y = c - ax/b$) can be demonstrated with a few simple algebraic steps as follows: Given, $y = c - ax/b$, a value for x can be found by squaring y and substituting it into equation 12 ($axx + byy = bcc$). This yields equation 11 ($x= 2cb/(b+a)$. We then substitute this value of x into equation 7 ($y = c - ax/b$), which yields $y = c - a(2cb/(b+a))/b$. Since we want y to be positive, let us assume that $y > 0$. So, $0 < (c - (2abc/(bb+ab)))$. This becomes, $c > 2abc/(bb+ab)$, which simplifies to $c(bb + ab) > 2abc$; $bb + ab > 2ab$;

the overlapping and competing geometric and algebraic representations of speed. Geometric diagrams, such as those in figure 1 for example, represent speeds as line segments. Represented as such, speed in any direction is a positive magnitude.

The calculations on the manuscripts suggest that Huygens had a reluctance to speculate about the existence of negative speeds. Despite the expanded domain of mathematical possibilities with the use of algebra, the specific equations that he repeatedly derived and utilized appear to be tailored to avoid negative results. In addition, Huygens's treatment and criticism of the Cartesian conservation of quantity of motion in the early manuscripts from the 1650s use only positive values. At this time he did not amend it by including negative numbers; rather he introduced an entirely new conservation principle; "body times speed squared." Whereas in 1669, when Huygens published some of his thoughts on collision in the *Journal des sçavans*, he would take direction into account. Huygens could show as early as 1652 that Descartes's "quantity of motion" can increase or decrease in collisions, and therefore is not conserved. In the *Journal des sçavans,* Huygens presented a new proposition, which may well stem from his considerations of positive and negative signs and direction, that states that "quantity of motion" taken toward one side (*i.e. with direction*) is conserved.[381]

On the 1652 manuscript, Huygens *twice* derived an equation for the acquired speed of the body *initially at rest*—namely equation 5 ($2ca/(b+a) = x$) and equation 11 ($x = 2cb/(b+a)$)—rather than derive the corresponding equation for the acquired speed of

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$bb - ab > 0$; $b(b - a) > 0$; $b > 0$; and $b > a$. This contradicts our previous definition of a as the larger body. Our assumption that $y > 0$ must therefore be false.

[381] HOC 16: 180. *La quantitié du mouvement qu'ont deux corps, se peut augmenter ou diminuer par leur rencontre; mais il y reste toûjours la mesme quantité vers le mesme costé, en soustrayant la quantité du mouvement contraire*

the body *initially in motion*. Equations 5 and 11 do not produce negative results, whereas the other corresponding equation would.

The equations on the 1652 manuscript, namely eq. 1): $bx + ay = ac$, and eq. 6): $ax + by = bc$, are surprisingly similar to the modern expression of the conservation of momentum (eq. 14). And Huygens's equation for the conservation of "body times speed squared," eq. 12): $axx + byy = bcc$ from the 1652 manuscript, is similar to modern expression of the conservation of kinetic energy (eq. 15):

$$m_1 v_{1i} + m_2 v_{2i} = m_1 v_{1f} + m_2 v_{2f} \qquad (14)$$

$$\tfrac{1}{2} m_1 v_{1i}^2 + \tfrac{1}{2} m_2 v_{2i}^2 = \tfrac{1}{2} m_1 v_{1f}^2 + \tfrac{1}{2} m_2 v_{2f}^2 \qquad (15)$$

More surprising is the *nearly* identical use of them to determine the final velocities of masses in what we call elastic collisions. Equations 14 and 15 are solved simultaneously to determine the final velocities of both objects:

$$v_{1f} = (\,(m_1 - m_2)/(\,m_1 + m_2)\,)\,v_{1i} + (\,2m_1/(\,m_1 + m_2)\,)\,v_{2i} \qquad (16)$$

$$v_{2f} = (\,2m_1/(\,m_1 + m_2)\,)\,v_{1i} + (\,(m_2 - m_1)/(\,m_1 + m_2)\,)\,v_{2i} \qquad (17)$$

When v_{2i}=0, the initial velocity of the second body is zero:

$$v_{1f} = (\,(m_1 - m_2)/(\,m_1 + m_2)\,)\,v_{1i} \qquad (18)$$

$$v_{2f} = (\,2m_1/(\,m_1 + m_2)\,)\,v_{1i} \qquad (19)$$

Compare equation 19 with the equations solved for x, such as equation 11 ($x=2cb/(b+a)$).

Nowhere on the 1652 manuscript does Huygens write or derive anything that corresponds to equation 18, an equation with potentially negative results. Notice that in equation 18, whenever m_2 is greater than m_1, v_{1f} is negative. Instead, he derives an equation similar to 19 twice, namely equation 5 in the first set and equation 11 in the second set of equations. And notice that in 19, as long as the body initially in motion is positive, v_{2f} will never be negative. Recall from section 2.2 that the duplicate sets of equations—instead of being entirely redundant derivations—correspond to two different kinds of physical events due to the referents of the symbols.

A similar tendency to avoid negative results is on display in his manuscript notes from 1654 as well.[382] Using the principle that the relative speed of approach is the same as the relative speed of separation, which appears verbally at the top of the 1652 manuscript and is later proven in proposition IV of *De motu corporum ex percussione*, the equations in the 1654 notes reveal an avoidance of negative algebraic results. See figure 2. Here both bodies are initially in motion, instead of one at rest. He represents y, the acquired speed of the larger body, as $c+dx$. This is derived from the principle that $c+d=x+y$. First, note the relationship between this algebraic expression, and the diagram of collision (see figure 10).

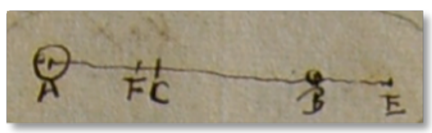

Figure 10

Let $AC=c$, $BC=d$, and $CA=x$, and $CB=y$.[383] Although c and d are in different directions—as are x and y—they are both depicted as positive magnitudes in the algebraic relation. (The sum $c+d$ represents the relative speed of approach, and the sum $x+y$ represents the relative speed of separation). Second, note what Huygens does to this value of y (the acquired speed of A), which of course, in the realm of possibilities could be negative. To find x he first squares the term for y, $(c + d - x)$, and multiplies it by a, which essentially removes the possibility of a negative. The value is then substituted into the equation *acc*

[382] HOC 16: 132-6.
[383] HOC 16: 132.

+ *bdd* = *ayy* +*bxx* for the term *ayy.*[384] He then solves for *x* to achieve the result, *x* = (2*ac* – *bd* + *ad*) / (*a* + *b*).[385] The squaring of a potentially negative result and substitution of it into the "body times speed squared" equation, you will recall, was Huygens's algebraic approach in 1652 as well. Although these examples reveal continuity in an attitude toward negative results, they also reveal larger trends in the two sets of notes. In 1652 the equations depict the Cartesian conservation of quantity of motion, whereas in 1654 Huygens has moved away from Cartesian relations and algebraically presents his own propositions and principles.

It is not entirely clear, given the evidence, how Huygens calculated the acquired speed of the body initially in motion. Given the steps he took to avoid negative results, it is unlikely that he would have used equations that would produce negative quantities for the speed. It is possible that he may have used his principle that the relative speed of approach is equal to the relative speed of separation, which is expressed algebraically in the manuscripts from 1654. Or Huygens may have drawn on his principle of relativity. If so, this would fit well with the notation he used in his doubly derived set of equations, both of which are solved for *x* (equations 5 and 11).

Beginning with his first investigation of collision in 1652, Huygens had claimed that motion is relative.[386] It is defined in relation to other bodies. Even the space in which a body moves can be considered to be in motion. In his first investigations in 1652, while

[384] Interestingly, Huygens appears to have attempted to use the principle that the speed of approach equals the speed of separation on the 1652 sheet as well. The equations are oriented upside-down from those described above, and are less orderly. Here he used *c-x* as the value for *y.* Since only one of the bodies is initially moving, *c*=*x*+*y* represents the principle that the speed of approach is equal to the speed of separation. However, instead of squaring *c-x*, it has been substituted into the quantity of motion equation, which does not appear to be fruitful.

[385] Note that *x* = (2*ac* – *bd* + *ad*) / (*a* + *b*) reduces to equation 5 (*x*=*2ac*/(*a*+*b*), when *d*=0.

[386] HOC 16: 93.

discussing the relativity of motion, he crossed out the word "space" and wrote "boat" to describe a moving space.[387] And in *De motu corporum ex percussione* Huygens would describe the collision of bodies as observed by a person on the shore, and as observed by a person on a boat, which could move at the same speed as one of colliding bodies. With this heuristic, and the principle of relativity, he could transform one scenario of colliding bodies into another.[388] In Huygens's equations in the 1652 manuscript, the symbol x in either equation refers to the acquired speed of the body initially at rest. In equation 5, x is the acquired speed of the smaller body initially at rest. In equation 11, x is the acquired speed of the larger body is initially at rest. Used in conjunction with a changing frame of reference, the two equations for x could solve for both bodies after collision.

In section 2.2, we saw that Huygens used symbolic equations to critically investigate Descartes's rules 4 and 5. His equations may well have served in private as an early argument against rule 4. The equations may have also been used to critically investigate Descartes's conservation principle. Descartes never algebraically presented his principle of the conservation of quantity of motion. Huygens did, and at least two problems would have immediately presented themselves. When values for x are substituted into an algebraic expression of the conservation of quantity of motion (*e.g.*, eq. 1: $bx + ay = ac$), negative numbers are produced whenever $bx/a > c$. In addition, and

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[387] HOC 16: 93. "But [let us also imagine] that the space itself is borne meanwhile to the left with the same said half-speed of the body B, thereby causing hose who are standing outside the space CDEF, say at H, to see A at rest and B moving, as was the case for both of them initially. Accordingly, after the collision those who are carried along with the space [*spatio*, above which Huygens wrote in the manuscript *navi*] CDEF will see B reflected to the right and A to the left, each of them with half the speed we attributed to B from the standpoint at H. But because the ship [*navis*] was assumed to move to the left with the same half speed, it will seem, viewed from H, that B is at rest, with A now moving to the left with the speed with which B moved initially." Translation by Gabbey, "Huygens and mechanics," 178.
[388] HOC 16: 32-33.

more damaging, the algebraic expression clearly shows that, given scalar values, the Cartesian quantity of motion could remain the same, decrease, or increase in collisions. Huygens provided a set of numerical examples in the 1652 manuscript that, when substituted into the algebraic expressions of Cartesian conservation of quantity of motion, numerically show that it is not conserved.[389]

By the late 1660s, Huygens had published summary accounts of his rules of motion with the Royal Academy of Sciences in France and the Royal Society in England.[390] In both he had become more explicit regarding change of direction. Descartes's conservation law, he maintained, was still not valid. But, if revised to focus on direction to one side, where the contrary direction was subtracted from the former, Cartesian quantity of motion was conserved. In the fifth proposition he wrote:

> The quantity of motion that two bodies have can be increased or decreased by their impact, but there will always remain the same quantity toward the same side by subtracting the contrary quantity of motion.[391]

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389 HOC 16: 96.

390 The former, recorded on 18 March 1669, was published in the *Journal des sçavans* 2 (1667-71): 531-6. It has also been collected in HOC 16: 179-81, (also see HOC 6: 383-5, Huygens to Gallois, 18 March 1669). The title in the *Journal des sçavans* is "Extrait d'une Lettre... " However, Huygens appears to have given the document the following title: "Regles du mouvement dans la rencontre des Corps." The latter summary of the treatise, recorded as 12 April 1669, was published in the *Philosophical Transactions* 4 (1669): 925-8. It has also been collected in HOC 6: 429-33. The title in the *Philosophical Transactions* is "A Summary Account of the Laws of Motion..." However, Huygens appears to have given the document the following title: "Regulæ de Motu Corporum ex mutuo impulsu." Note that Huygens consistently referred to the document "rules of motion," rather than "laws of motion," as the editor of the *Philosophical Transactions* did. The version printed in the *Journal des sçavans* is slightly different from the paper originally submitted to the Royal Society. See A. Rupert Hall, "Mechanics and the Royal Society, 1668-70," *The British Journal for the History of Science* 3 (1966): 33-5. Huygens also sent demonstrations for several of his proposition to the Royal Society on 5 January 1668/9, which were not published in the *Philosophical Transactions.* See HOC 6: 336-43.

391 HOC 16: 180. Translation by Iltis, "The Controversy over Living Force: Leibniz to D'Alembert" (PhD diss., University of Wisconsin, Madison, 1967), 48-50. Curtis Wilson provides an English translation of the Latin paper (which Oldenburg had translated from the original French paper) published in the *Philosophical Transactions*, "*Regulæ de motu corporum ex mutuo impulsu*." Gemma Murray, William Harper, and Curtis Wilson, "Huygens, Wren, Wallis, and Newton on Rules of Impact and Reflection," in *Vanishing Matter and the Laws of Motion: Descartes and Beyond*, ed. Dana Jalobeanu and Peter R. Anstey (New York: Routledge, 2011) 154-7.

It is plausible that the origin of this new principle emerged out of Huygens's struggles with negative values, which began in the early 1650s as sophisticated attempts to avoid negative results. Quantities less than zero were suspect generally among many mathematicians of the time. For Huygens, they may have conflicted with the notion of "positive" magnitudes depicted by the line segments of diagrams, and the physical quantities they represented. Although not a full acceptance of "negative speeds," the rule mentioned above does utilize the notion of subtraction to indicate contrary direction: "there will always remain the same quantity toward the same side by subtracting the contrary quantity of motion."

Section 3
Huygens's pendulum and collision

The pendulum played an important role in Huygens's early work on collision. It served as an opportunity and means for Huygens to make successful predictions, which persuaded his English colleagues that his theory was correct, although empirical adequacy alone was not a satisfactory justification, as we will see in more detail in the following chapter on collision in the Royal Society. The pendulum was a tool for both empirical investigation and for conceptual analysis. Several of Huygens's *principles* likely originated from his principles of pendular motion. These include his variant of the "Torricelli's principle," which Huygens's used extensively throughout his work, the principle of the reversibility of impact, and his principle of the conservation of "body times speed squared."

Corresponding in time to Huygens's critical investigation of collision is his early work on the pendulum. In 1646 Mersenne had posed a question – to determine of the center of oscillation of the compound pendulum – to several of his correspondents which included the then young Huygens as well as Cavalieri, Torricelli, Baliani, Descartes, and Roberval.[392] Although the 17 year-old Huygens did not solve the problem immediately, he would include a solution and proof to the problem in the fourth section of what has been called "one of the masterpieces of seventeenth-century scientific literature," the *Horologium Oscillatorium*, which had been started at least by 1659, but was not published until 1673.[393] Throughout his life Huygens concerned himself with the principles of the pendulum and its applications, extending and amending Galileo's work on isochronous pendulums. By 1657 Huygens had already invented his first mechanical pendulum clock, and by 1661 he had "discovered the tautochrony of the cycloidal pendulum, the cycloidal cheeks that make a pendulum cycloidal, the conical pendulum, the center of oscillation of a compound pendulum, and the sliding weight to vary the pendulum's period."[394] He had determined the mechanical and geometrical properties of the cycloid, which is the ideal shape for the metal cheeks that he made to regulate the swing of the pendulum. Constraining the oscillations in this way makes the period independent of the amplitude, and thus isochronous. He also investigated the mathematical theory of evolutes more generally. Huygens produced theorems of uniform circular motion and centrifugal force through his work on the circular pendulum,

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[392] Bertoloni Meli, *Thinking with Objects*, 117. A simple pendulum is an ideal machine in which the entire mass is concentrated at one point in the bob and the mass of the material suspending the bob is disregarded. A compound pendulum, on the other hand, is a swinging rigid mass, such as a metal rod, suspended from some point. "The center of oscillation is that point on the line from the suspension point to the center of gravity, whose distance from the suspension point is equal to the length of a simple pendulum with the same period."

[393] Bertoloni Meli, *Thinking with Objects*, 116-117. Gabbey, "Huygens and mechanics," 173. Joella Yoder, *Unrolling Time: Christiaan Huygens and the mathematization of nature* (Cambridge: Cambridge University Press, 1998), 2-3.

[394] Michael Mahoney, "Christiaan Huygens: The measurement of time and longitude at sea," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980), 236.

proposed using the seconds pendulum as a universal measure of length, and determined the acceleration of gravity by pendulum observations.[395]

Several images of pendulums can be found accompanying the algebraic equations, diagrams of collision, and preliminary propositions on the 1652 manuscript. For instance, a small drawing in the upper left of figure 1, reproduced in figure 11, depicts what appears to be two pendulums with unequally sized bobs either just approaching or just separating from a collision.

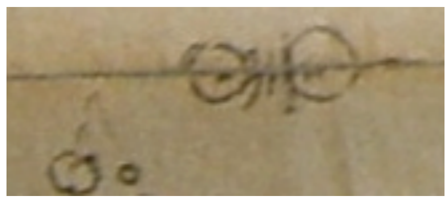

Figure 11

In the lower right and upside down in figure 1, reproduced in figure 12, is what appears to be the arc swept out by a pendulum with various indications of heights along a vertical line. The heights of the pendulum bobs before and after collision could be used as an empirical measure of speed. Just to the left, but oriented orthogonally to the pendulum is a diagram of a larger body *A* colliding with smaller body *B*. Just above is what appears to be two unequal bodies on a circular track, possibly representing “the reversibility of collisions” or perhaps experiments with a circular pendulum.

[395] Bertoloni Meli, *Thinking with Objects*, 205-18. Mach, *The Science of Mechanics*, 155-87. Mahoney, "Measurement of time and longitude," 234-70. Yoder, *Unrolling Time*.

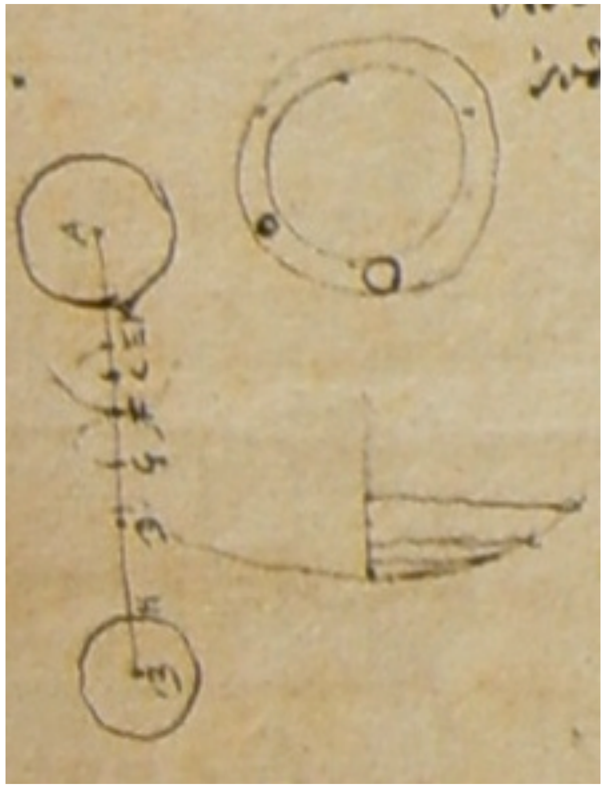

Figure 12

The "Torricelli principle"[396] states that two connected bodies cannot move by themselves unless their common center of gravity descends.[397] According to some commentators, the Torricelli principle is "arguably the most important single axiom in [Huygens] entire mechanics" and is used throughout his work in various formulations and transformations.[398] Dijksterhuis—who in addition to writing the classic *The Mechanization of the World Picture* in which Huygens features prominently, also served

[396] Pierre Duhem, *Les Origines de la statique*, vol. 2 (Paris: Librairie Scientifique A. Hermann, 1906), 6. Also see Gabbey, "Huygens and mechanics," 168. Both Gabbey and Duhem state that the principle was of ancient lineage and is referred to as "Torricelli's Principle" simply for convenience. For Huygens's uses of the Torricelli principle see the editor's introduction to HOC 16: 21-5. Also see Dijksterhuis, *Mechanization* (IV: 141-2), 370-3, and Bertoloni Meli, *Thinking with Objects*, 232-3.

[397] Evangelista Torricelli, *De motu gravium naturaliter descendentium et projectorum* (Florence, 1644), in *Opere de Evangelista Torricelli*, vol. 2, ed. Gino Loria and Giuseppe Vassura (Faenza: G. Montanavi, 1919-1944) 105, 108-9. Bertoloni Meli, *Thining with Objects*, 119. Also see Christiane Vilain, "Christiaan Huygens' Galilean Mechanics," in *The Reception of the Galilean Science of Motion in Seventeenth-century Europe,* ed. Carla Rita Palmerino and Thijssen (Boston: Kluwer Academic Publishers, 2004), 195.

[398] Gabbey, "Huygens and mechanics," 168.

as an editor of the *Oeuvres Complètes De Christiaan Huygens*—describes one of Huygens's "generalizations" and applications of the Torricelli principle. The axiom, which was originally static and formulated for equilibrium, was "transform[ed] into an extremely fundamental dynamical principle,"[399] and was applied to the problem of the center of oscillation of a compound pendulum.[400] He describes Huygens's transformed Torricelli principle as follows:

> We first imagine a particle, which is kept in a state of rest at a height *h* [...] when released, it will begin to move. Its height h_1 after a given time will be less than in the initial position, but it has now acquired a certain velocity. Let us suppose that it can use this velocity to rise vertically and that in consequence it will rise over a distance of h_2. It will then be at a height h_1+h_2. Now Huygens states it as an axiom that this height cannot be greater than *h*. If one assumes that the motion in question can also take place in the reverse direction, *h* in turn cannot be greater than h_1+h_2, so we have $h=h_1+h_2$. [...] If we now have a system of interconnected particles and if in this system at a given instant all the particles, independently of each other (*i.e.* the connexions [*sic*] being broken), are made to perform the upward motion just described, their common centre of gravity will always return to its original height.[401]

In the context of his theory of collision, Huygens uses a variant of the Torricelli principle similar to that mentioned above—one which is particularly suited to the use of pendulum bobs. In the original Torricelli principle, the two bodies are *connected*, and do not move unless their common center of gravity descends. Huygens's key modification is to "disconnect" the bodies and to consider them in motion. He claims that if the line along which the colliding bodies move is converted from the horizontal to the vertical, then the

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[399] Dijksterhuis, *Mechanization* (IV: 141), 370. Also see HOC 16: 21, the editor's *Advertissement* for the *De motu corporum ex percussione* treatise and associated manuscripts.

[400] Dijksterhuis, *Mechanization* (IV: 142), 371-3. Also see HOC 16: 421, *Travaux divers de Statique & de Dynamique de 1659 à 1661*. Gabbey cites another of Huygens's formulations and applications of the Torricelli principle. *De iis quae liquido supernatant, Libri 3* was written in 1650, but never published (at least until 1908 in the OC). Here Huygens applied "Torricelli's Principle to prove firstly Archimedes' Law of floating bodies, and then to prove the general theorem that for a floating body in equilibrium the distance between the centres of gravity of the body and of its submerged portion is a minimum." See Gabbey, "Huygens and mechanics," 168.

[401] Dijksterhuis, *Mechanization* (IV: 141), 370-1.

center of gravity of the system of disconnected, but colliding, bodies cannot rise after impact. In colliding pendulums horizontal motion is converted to vertical motion.[402]

The principle of the reversibility of motions states that "if two bodies collide again with the speeds they have acquired after the first impact, they will acquire the same speeds they had before the first impact."[403] After two pendulum bobs collide they will rebound with their respective acquired speeds to their respective heights. Unless the observer intervenes in some way, the bobs will descend after reaching the heights corresponding to the acquired speeds, and collide again.

The principle of the conservation of "body times speed squared" appears on Huygens's 1652 manuscripts. Ernst Mach referred to this principle—however *not* in the context of collision but rather in the context of the compound pendulum—by the name it would later acquire, the principle of *vis viva*. Mach claimed that the principle Huygens used in his solution to the center of oscillation of a compound pendulum, is *identical* with the principle of *vis viva*.[404] Mersenne had posed the problem of the compound pendulum[405] in 1646, and Huygens provided his solution and proof in his *Horologium oscillatorium*.[406] Mach calls the principle used to solve the problem, "Huygens's crowning achievement."[407] However, Mach's point seems to be that "Huygens's principle"—which is the variant of the Torricelli principle found in the Dijksterhuis quote

[402] HOC 16: 21-5, 95n. Bertoloni Meli, *Thinking with Objects*, 233. Vilain, "Huygens' Galilean Mechanics," 195.
[403] Bertoloni Meli, *Thinking with Objects*, 232. Also see HOC 16: 46-7.
[404] Mach, *The Science of Mechanics*, 178.
[405] An example of a compound pendulum is a swinging metal rod. The bulk (or mass) is distributed along its length rather than isolated only in the bob.
[406] Yoder, *Unrolling Time*, 156-7. Huygens had started and abandoned the problem in 1659 as well as 1661, before taking it on again in 1664.
[407] Mach, *The Science of Mechanics*, 187.

above[408]—can be shown to be equivalent to the conservation of kinetic energy of a system of masses.[409] Mach neglected to mention that the principle Huygens used to solve the center of oscillation problem is the same as the principle of "body times speed squared" which Huygens explicitly used even in his earliest works on collision such as the manuscript from 1652.

The origins of Huygens's conservation principle of "body times speed squared" likely stem from a creative synthesis of Huygens's variant of the Torricelli principle for colliding pendulum bobs, the reversibility of collisions, and the extensions of Galileo's law of fall to fall along a circular curve. According to Huygens's variant of the Torricelli principle, if the line along which the colliding bodies move is converted from the horizontal to the vertical, then the center of gravity of the system of disconnected bodies cannot rise after impact. Using this and the reversibility of collisions, it can be shown that when two hard pendulum bobs collide, the center of gravity of the system will neither ascend to a greater height, nor to a height lower than it initially was. That it cannot ascend higher follows immediately from the Torricelli principle. It will not go to a lower

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[408] Mach, *The Science of Mechanics*, 174-5. "The *new* idea from which Huygens set out, and which is more important by far than the whole problem, is this. In whatsoever manner the material particles of a pendulum may by mutual interaction modify each other's motions, in every case the velocities acquired in the descent of the pendulum can be such only that by virtue of them the centre of gravity of the particles, whether still in connection or with their connections dissolved, is able to rise just as high as the point from which it *fell*. Huygens found himself compelled, by the doubts of his contemporaries as to the correctness of this principle, to remark, that the only assumption implied in the principle is, that heavy bodies of themselves do not move upwards. If it were possible for the centre of gravity of a connected system of falling material particles to rise higher after the dissolution of its connections than the point from which it had fallen, then by repeating the process heavy bodies could, by virtue of their own weights, be made to rise to any height we wished. If after the dissolution of the connections the centre of gravity should rise to a height less than that from which it had fallen, we should only have to reverse the motion to produce the same result. What Huygens asserted, therefore, no one had ever really doubted; on the contrary, every one had instinctively perceived it. Huygens, however, gave this instinctive perception an *abstract, conceptual* form."

[409] Mach, *The Science of Mechanics*, 178. "We see without difficulty in the Huygenian principle the recognition of *work* as the condition *determinative of velocity*, or, more exactly, the condition determinative of the so-called *vis viva*. By the *vis viva* or living force of a system of masses *m, m,, m,,,...,* affected with the velocities *v, v,, v,,,....* we understand the sum $mv^2/2+m_,v_,^2/2+m_{,,}v_{,,}^2/2+\ldots$ The fundamental principle of Huygens is identical with the principle of *vis viva*."

height for the following reason. Imagine that it was possible for the center of gravity of to go to a lower height after collision. By the reversibility of collisions, the bodies moving again after impact, now with their acquired speeds, would subsequently acquire the same speeds they had before impact. This means that they would ascend to their original (higher) height, but this would make the center of gravity ascend higher, which is impossible. So, the height to which the bobs return must be the same. According to Galileo's law of fall, the height is proportional to speed squared. Thus we can see how one may arrive at the relation that the speed squared of the pendulum bob neither increases nor decreases. It is conserved.

Section 4
Huygens's axiomatic formulation: "hypotheses" and symmetry

In Huygens's earliest work on collision from 1652 he made innovative use of symbolic algebra in an exploratory analysis and criticism of Descartes's rules of collision. His work with algebra may have also contributed to the development of a new principle, the conservation of Cartesian quantity of motion with direction. Huygens ideas on the pendulum were also important to the development of his theory of collision, namely his variant of the Torricelli principle, the reversibility of impact, and the conservation of "body times speed squared." Huygens's investigation of collision was initially inspired by Descartes's rules of collision, and even his early challenges of Descartes's rules relied on Descartes's symbolic algebra. However, the model for Huygens's treatise, *De motu corporum ex percussione* was Galileo's *Discorsi*.[410] Huygens formulated his argument in

[410] Westfall, *Force in Newton's Physics*, 153.

the axiomatic tradition of Archimedes and Galileo, relying on principles of symmetry, as well as concepts of motion and rebound that were foreign to Descartes's system.

Huygens's *Regulæ de motu corporum ex mutuo impulsu* was published in the *Philosophical Transactions* in 1669.[411] It was a Latin translation of his *Regles du mouvement dans la rencontre des corps*, which was published in the *Journal de Sçavans* a few months earlier.[412] These papers were a summary of the more extensive theory of collision that Huygens had been developing since 1652, culminating in 1656 as *De motu corporum ex percussione*,[413] but which would not be published until 1703 (8 years after Huygens died), possibly because Huygens was continually refining the treatise and searching for a better axiomatic presentation.[414]

The first two propositions in the *Regles du mouvement dans la rencontre des corps* are essentially the same as the first two propositions in the more extensive *De motu corporum ex percussione.* (1) "When a hard body directly encounters another hard body equal to it and at rest, it gives all its motion to it, itself remaining motionless after collision." (2) "But if this other equal body is also in motion and when it moves in the same straight line, they reciprocally exchange their motions."[415] However, in *De motu corporum ex percussione* they are demonstrated from other more fundamental axioms (or as Huygens's calls them "hypotheses"). The third proposition in both texts directly

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[411] Christiaan Huygens, "Regulæ de Motu Corporum ex mutuo impulsu," *Philosophical Transactions* 4 (1669): 925-8. It is also collected in HOC 6: 429-33.
[412] Christiaan Huygens, "Regles du mouuement dans la rencontre des corps," *Journal des sçavans* 2 (1667-71): 531-6. It is also collected in HOC 16: 179-81 (also see HOC 6: 383-5, Huygens to Gallois, 18 March 1669). The version in the *Journal des sçavans* was slightly different from the paper submitted to the Royal Society. See Hall, "Mechanics and the Royal Society," 33-35.
[413] HOC 16: 30-91.
[414] Westfall, *Force in Newton's Physics*, 146-58. Westfall provides an account of the development of Huygens's work on collision from 1652 to its final form in *De motu corporum ex percussione* 1656.
[415] HOC 16: 179. Translation by Iltis, "Controversy over Living Force," 48. Also see Murray et al., "Huygens, Wren, Wallis, and Newton," 154-7.

contradicts Descartes's fourth rule. In the *Regles* it is stated as follows: (3) "A body somewhat smaller than it and having somewhat less velocity, in encountering another greater and at rest will give some of its motion to it." [416] *De motu* provides a demonstration. The fourth proposition in the *Regles* is a "general rule for determining the motion which hard bodies acquire by direct impact."[417] And the fifth proposition in the *Regles* directly contradicts the fundamental Cartesian principle, the conservation of Cartesian quantity of motion.

> (5) The quantity of motion that two bodies have can be increased or decreased by their impact, but there will always remain the same quantity toward the same side by subtracting the contrary quantity of motion.[418]

The first part of this statement (excluding the comment regarding subtraction) corresponds to Proposition VI in *De motu corporum ex percussione*, and, like the other propositions in this treatise, are demonstrated from prior propositions and hypotheses. The sixth proposition in the summary is the conservation principle with which Huygens replaces the Cartesian principle, the conservation of body times speed squared. (6) "The sum of the products of the size of each hard body multiplied by the square of its velocity is always the same before and after impact." The seventh proposition involves increasing the motion a body at rest will receive by interposing a third body in between the two. And lastly there is an unnumbered proposition which states that "the common center of gravity of two or three (or such as one wishes) bodies, always advances equally toward the same side in a straight line before and after impact."[419]

I would like to highlight the first three hypotheses (axioms), which Huygens used

[416] HOC 16: 180. Translation by Iltis, "Controversy over Living Force," 48.
[417] The fourth corresponds to Proposition IX in *De motu corporum ex percussione*. See HOC 16: 65.
[418] HOC 16: 180. Translation by Iltis, "Controversy over Living Force," 48-50.
[419] HOC 16: 180. Translation by Iltis, "Controversy over Living Force," 50.

in his demonstrations of the propositions regarding the collision of two equal bodies (*i.e.* the first two propositions mentioned above). Huygens was working in the axiomatic tradition of Archimedes's *On the equilibrium of planes* and Galileo's *Two New Sciences*. As such, the *hypotheses* are "principles to which the mind naturally consents."[420] The first hypothesis has been called the "principle of inertia."[421] It states that "any body once moved continues to move, if nothing prevents it, at the same constant speed and along a straight line."[422] The second two hypotheses are instrumental in using the concept of symmetry to establish Huygens's theory of collision.

The second hypothesis states that "when two equal bodies with equal speed collide directly with one another from opposite directions each rebounds with the same speed with which it approached."[423] It has often been remarked, whether when discussing Descartes's version of this rule or Huygens's, that this scenario describes a "symmetric" case of collision.[424] There are a variety of apparent "symmetries" in the hypothesis. There is a spatial bilateral symmetry if the point of impact is taken as the axis. This echoes the equilibrium of the balance when the weights and arms are the same, as enunciated by Archimedes. If the collision itself is taken to be the transformation, the relative speed of approach is the same as the relative speed of separation, *i.e.* the relative speed is invariant. Unlike other various instances of collision, it seems immediately apparent in the case described in hypothesis two—equal hard bodies colliding with equal speeds and rebounding with the same speed—that the relative speed is invariant. Huygens would go

[420] Domenico Bertoloni Meli, "The Axiomatic Tradition in 17th-Century Mechanics," in *Synthesis and the Growth of Knowledge*, ed. M. Dickson and M. Domski (Chicago: Open Court, 2010) 23, 33-35. Also see Bertoloni Meli, *Thinking with Objects*, 66, 96, 232-3. Westfall, *Force in Newton's Physics*, 153.
[421] Westfall, *Force in Newton's Physics*, 153.
[422] HOC 16: 31. Translation by Mahoney, "On the motion of bodies resulting from impact."
[423] HOC 16: 31. Translation by Mahoney, "On the motion of bodies resulting from impact."
[424] Westfall, *Force in Newton's Physics*, 148, 153.

on to prove that the relative speed of approach is equal to that of separation *in general* as well.[425]

> The third hypothesis is "the principle of relativity:"
>
> The motion of bodies and their equal and unequal speeds are to be understood respectively, in relation to other bodies which are considered as at rest, even though perhaps both the former and the latter are involved in another common motion. And accordingly, when two bodies collide with one another, even if both together are further subject to another uniform motion, they will move each other with respect to a body that is carried by the same common motion no differently than if this motion extraneous to all were absent.
>
> Thus, if someone conveyed on a boat that is moving with a uniform motion were to cause equal balls to strike one another at equal speeds with respect to himself and the parts of the boat, we say that both should rebound also at equal speeds with respect to the same passenger, just as would clearly happen if he were to cause the same balls to collide at equal speeds in a boat at rest or while standing on the ground.[426]

Collision is not affected by the principle of relativity. The structure of collision remains invariant when the frame of reference changes between the "passenger on the boat" and the person on the shore. Using the "symmetric" case described in hypothesis two of equal bodies with equal speeds colliding and separating with the same speed, and the principle of relativity, Huygens derives any case of collision of equal bodies. For example, a person on a boat holds pendulum bobs in each hand. If he were to bring his hands together at a constant and equal speed, the bodies after collision would move with the same equal speeds in opposite directions (hypothesis 2). If the boat were to move at a constant speed equal to that with which the man on the boat moves one of the pendulum bobs, to a person on the shore, the pendulum bob would appear to be at rest. And if the two men were to touch hands as the man on the boat brought the pendulum bobs together

[425] HOC 16: 43-5. That "the relative speed of approach is equal to the relative speed of separation" is demonstrated generally for any collision of hard bodies in Proposition IV of *De motu corporum ex percussione.*
[426] HOC 16: 33. Translation by Mahoney, "On the motion of bodies resulting from impact."

in accordance with hypothesis 2, the person on the shore would affectively hold one bob at rest and move the other bob toward it.

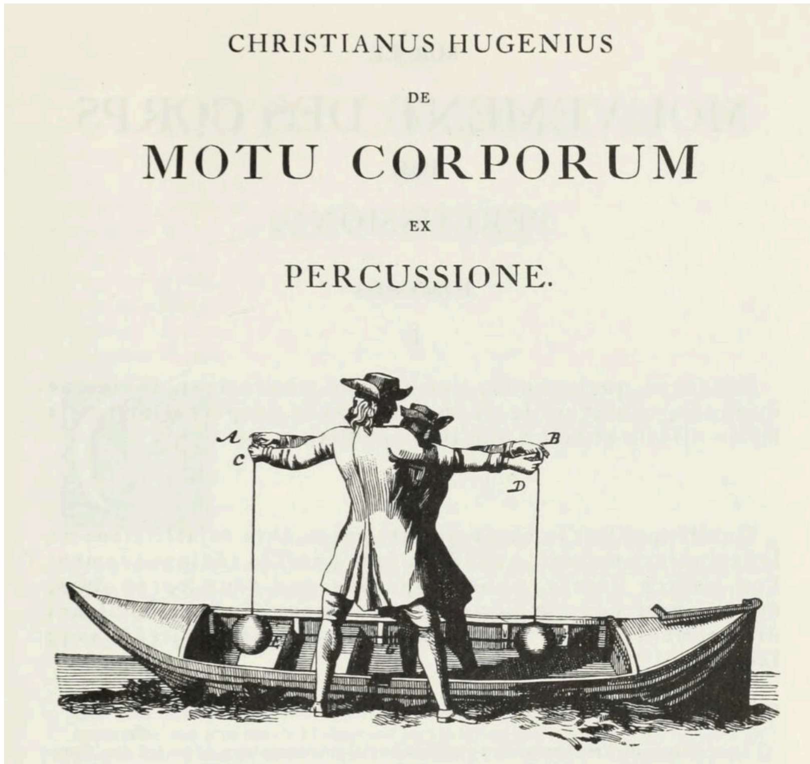

Figure 13. HOC 16: 29

A distinction can be noted between the "symmetrical" case of collision (hypothesis 2), and the role of "symmetry" in Huygens's use of the principle of relativity in conjunction with his axiomatic case of collision in his demonstrations. The former symmetry is a property of an entity; the latter symmetry is a relation. Hypothesis 2 describes "a whole whose parts are in agreeable proportion." The use of hypothesis 2 along with 3 (the relativity principle) to establish other cases of collision is an operation—a transformation in which something remains invariant.[427] The latter notion of

[427] The roles of symmetry in the rules of collision will be discussed in greater length in chapter 6. Also see Giora Hon and Bernard R. Goldstein, *From Summetria to Symmetry: The making of a revolutionary scientific concept*, in *Archimedes: New Studies in the History and Philosophy of Science and Technology*, vol. 20, ed. Jed Buchwald (New York: Springer, 2008). And see György Darvas, *Symmetry: Cultural-historical and ontological aspects of science-arts relations; the natural and man-made world in an*

symmetry is of primary importance in Huygens's theory. And, as we will see in chapter 5, it is the former notion of symmetry that plays a significant role in Wren's theory of collision.[428]

4.1 – Challenging Descartes *without* Cartesian concepts: relativity and rebound

From the beginning of Huygens's studies of collision in the early 1650s, he was committed to refuting Descartes's rules. Huygens persisted in this despite the disapproval of his teacher, the Cartesian, Frans van Schooten, who encouraged him to abandon the project.[429] Descartes's rules conflicted with experience. Huygens's did not, as Huygens pointed out to his correspondents, and as he would show through his successful predictions in 1661 in London. However, Huygens's criticisms were not merely that Descartes's rules did not match experience. Descartes had acknowledged this much himself in the *Principles of Philosophy*, as we saw in the previous chapter, and Huygens was cautious about relying too heavily on experience, which can be uncertain, or in his words, "slippery."[430] Huygens's arguments were mathematical and "rational" rather than empirical. The arguments were at least in part from a position internal to Descartes's system of ideas. Huygens used Descartes's mathematics against Descartes's physical

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interdisciplinary approach, trans. David Robert Evans (Boston: Birkhäuser, 2007). Hon and Goldstein had drawn a distinction between symmetry as a property of an entity and symmetry as a relation. The forcefully argue that the latter did not emerge until 1794 with Legendre's "revolutionary definition" of symmetry in his *Élements de géometrie.* Darvas distinguishes between symmetry as a phenomenon, concept, and operation. However, he does not share Hon and Goldstein's view, and argues that symmetry is a rich and fundamental concept that bridges times, cultures, and disciplines. Both recognize the differing historical place of symmetry in an aesthetic context and a "group theoretic" context.

428 It can be found in the notion of a balance in equilibrium in his theory as well as the diagrammatic presentation of his theory which is organized symmetrically, and which is meant to be read both in the "Latin way" (from left to right) as well as the "Hebraic" (from right to left). Symmetry in Wren's theory works in tandem with the values put on brevity, unity, and the economy of the algebraic symbol.

429 Richard Westfall, *Force in Newton's Physics* (New York: American Elsevier, 1971), 147.

430 HOC 2: 79-80, 114-5. Christiaan Huygens to R. F. de Sluse, 2 November 1657. Also see Bertoloni Meli, *Thinking with Objects*, 233.

rules of collision, as we have seen above in section 2.1. And, in the treatise prepared for publication, Huygens used Descartes's first rule—that two equal bodies moving toward each other with the same speed will reflect and move away from each other with the same speed[431]—and the principle of the relativity of motion (which at least *seems* to be similar to Descartes's notion of motion) to argue that the Cartesian rules are inconsistent and that the Cartesian "quantity of motion" is not actually conserved.[432] However, Huygens disregarded fundamental components of Descartes's system. Huygens's understanding of motion, particularly his notion of relative motion, was *not* Cartesian, but was likely an "extension" of Galilean relativity.[433] And Huygens's understanding of bodies, particularly their relationship to rebound, is in sharp contrast to Descartes's understanding of bodies as well as Descartes's efforts to explain the conditions of rebound and the transfer of motion.

In the *Principles of Philosophy* Descartes had described a "vulgar" and a "proper" conception of motion. The vulgar conception "is nothing other than *the action by which some body travels from one place to another.*"[434] Under this conception, Descartes shows that the same thing can be said "to move and not to move."

> Thus a man, seated in a ship which is sailing out of port, thinks that he is moving if he turns his attention to the shores, which he considers to be at rest. But he does not think so if he turns his attention to the parts of the ship, in relation to which he constantly maintains the same situation.[435]

But this is only motion "as commonly interpreted." Descartes's "proper" conception of motion, which is "in accordance with the truth of the matter," attributes to motion some

[431] AT VIII 68. *Principia* II 46.
[432] Westfall, *Force in Newton's Physics,* 148-58. Compare this to Dijksterhuis's rather anachronistic and abbreviated account of Huygens's use of relativity in *De motu corporum ex percussione*. See Dijksterhuis, *Mechanization* (IV: 143-4), 374-5.
[433] Vilain, "Huygens' Galilean Mechanics," 194-7. Gabbey, "Huygens and mechanics," 179.
[434] AT VIII 53. *Principia* II 24. Translation by Miller, *Principles*, 50.
[435] Ibid.

"determinate nature." By shifting the focus from a "change of place" to the "transference with respect to the immediate neighborhood," Descartes's "proper" conception was intended to limit the arbitrariness of whether a thing is in motion or not.[436]

> [Motion] is *the transference [translatio] of one part of matter or of one body from the neighborhood [vicinia] of those bodies that immediately touch it and are regarded as being at rest, and into the neighborhood of others.*[437]

However, Descartes's also acknowledged that it is impossible to know whether the neighborhood moves with respect to the body or the body moves with respect to the neighborhood. Transference is reciprocal.[438] Some commentators have claimed that this is evidence that Descartes's proper conception of motion is ultimately relative. Garber, on the other hand, has argued that even with the "reciprocity of transfer" Descartes provided a non-arbitrary distinction between motion and rest. Motion is the mutual separation of a body and its neighborhood.[439]

Whether or not the "reciprocity of transfer" entails some form of relative motion, Descartes's conception of motion is clearly different from Huygens's. Rather than the "vulgar" notion of motion in which the observer's perception determines whether a body is in motion, Descartes defined a "proper" conception of motion, which privileged the reciprocal transfer of a body and its neighborhood. Huygens, on the other hand "believed from the beginning of his career not only that the position of the observer influenced the perception of motion, but also that there was no privileged point of view, for all

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[436] Garber, *Descartes' metaphysical physics*, 162-72.
[437] AT VIII 53. *Principia* II 25. Translation by Garber, *Descartes' metaphysical physics*, 159-60.
[438] AT VIII 55-6. *Principia* II 29. "Finally, I added that the transference thake place from the neighborhood not only of any contiguous bodies, but only from the neighborhood *of those regarded as being at rest.* For that transference is reciprocal, and we cannot understand body AB transferred from the neighborhood of body CD unless at the same time body CD is also transferred from the neighborhood of body AB." Translation by Garber, *Descartes' metaphysical physics*, 166-7.
[439] Garber, *Descartes' metaphysical physics*, 168.

viewpoints were equivalent."[440] Huygens's relativity principle may have been inspired by Galileo, whom Huygens greatly admired, more so than Descartes.[441] But even so, Huygens's principle of relativity was far more radical.[442]

Huygens's position on collision relies on a concept of motion that Descartes did not share. In addition, Huygens's understanding of the bodies involved in impact differs from those of Descartes, as does Huygens's account of rebound. Descartes had identified body with extension and maintained that the world was a plenum, whereas Huygens accepted inter-particle vacua, and noted that bodies with the same volume have different densities, thus their size is different from their bulk.[443] While Descartes and Huygens both described the bodies involved in collision as perfectly “hard” or “solid,” their notions of a “hard body” differ significantly. Descartes notion of hardness was defined as the mutual rest of the "parts" of a body.[444] However, I contend that whether or not the bodies *in Descartes's rules of collision* rebound is *not* determined by the nature of the body as hard or soft, solid or fluid. In his rules of collision, Descartes explained rebound

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[440] Vilain, "Huygens' Galilean Mechanics," 196. Also see Christiane Vilain, "Huygens and Relative Motion," in *Relativity in General: Proceedings of the Relativity Meeting ‘93*, ed. Diaz Alonzo and M Lorente Paramo (Gif-sur-Yvette Cedex: Atlantica Seguier Frontieres, 1995), 161-9.

[441] Gabbey, "Huygens and mechanics," 175-81. Alan Gabbey has suggested that Huygens’s relativity principle did not originate with Descartes, but rather with Galileo. According to Gabbey, *Galilean* relativity involved “dynamic considerations” which Huygens too had used in his earliest work on collision. Huygens initially appealed to a force, the *vis collisionis*, in his 1654 notes on collision, and used the invariance of *forces* with respect to frames of reference in his early collision theory. Huygens would later abandon the *vis collisionis*, but it was present in his first arguments using relativity. Compare this with Westfall, *Force in Newton's Physics*, 150-1. In his chapter, “Christiaan Huygens’ Kinematics,” Westfall also argues that Huygens’s work on collision began with a notion of force that was later abandoned. However, unlike Gabbey, Westfall claims that Huygens used Descartes’s relativity principle against Descartes: “Here, of course, was the principle of the relativity of motion, Descartes’ own principle turned against his own conclusions.”

[442] Vilain, "Huygens' Galilean Mechanics," 196. Dijksterhuis, *Mechanization* (IV: 149), 378. HOC 16: 222. "True motion is relative motion."

[443] Gabbey, "Huygens and mechanics," 176. H. A. M. Snelders, "Christiaan Huygens and the concept of matter," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980) 110. Murray et al., "Huygens, Wren, Wallis, and Newton," 160.

[444] The word “parts” is in quotes, because if the so-called parts were at rest with respect to each other, the parts would be indistinguishable, since motion is what individuates bodies in Descartes’s plenum.

by his contest view of force: a body rebounds if it does not overcome to the force of resistance. For Huygens, rebound is *not* explained in terms of a contest between the moving force and the force of resistance. And hardness, at least the hardness of particles of matter, is an absolute and independent property.[445] The bodies described in Huygens's account of collision are not only perfectly hard, but they behave as (what we would call) a perfectly elastic body would.

Throughout *Le Monde* Descartes uses the terms “solid bodies” *corps solide* and “hard bodies” *corps durs* interchangeably,[446] just as “liquid bodies” *corps liquides* and “fluids” *liqueurs* are used interchangeably.[447] In the *Principles of Philosophy,* Descartes also uses both binaries, hard/liquid and solid/fluid.[448] However, his notion of a perfectly hard body is somewhat complicated by his notion of “solidity,” which he defines as “the quantity of the matter of the third element...in proportion to its [the body’s] volume and surface area”[449] (*e.g.* gold and lead have more solidity than wood or rocks).[450] However, immediately after the rules of collision are presented in the *Principles of Philosophy*, and as if to clarify what he meant by the "perfectly hard bodies" (*perfecte dura*) that are featured in his rules,[451] Descartes draws a fundamental distinction between a hard body and a fluid body and defines them respectively (*Quæ sint corpora dura, quæ fluida*). The parts of a perfectly solid body are at rest with respect to each other, and it is “rest” that

[445] Snelders, "Huygens and the concept of matter," 118.
[446] AT XI 12, 17. For “hard bodies,” see chapter 3 of *Le Monde*. For “solid bodies” see chapter 4.
[447] AT XI 24. For the former see chapter 3, for the latter see chapter 5.
[448] AT VIII 70. *Principia* II 54. “Solid/Fluid” is a basic distinction between different qualities of bodies, defined by the motions of their parts. AT VIII 211. *Principia* IV 19. When Descartes discusses “liquids,” he specifically describes the effects of terrestrial “actions” that produce various phenomena such as the roundness of drops of water. Thus, in the *Principles of Philosophy* “fluid” may refer to a basic quality of matter explained in terms of its parts, whereas “liquid” may refer specifically to a terrestrial phenomenon. See AT VIII 26, *Principia* I 56, for Descartes’s definitions of modes, qualities, and attributes.
[449] AT VIII 170-2. *Principia* III 121. Translation by Miller, *Principles*, 151-2. For a discussion of solidity see Bertoloni Meli, *Thinking with Objects*, 158-9.
[450] AT VIII 172. *Principia* III 122.
[451] AT VIII 67. *Principia* II 45.

binds them together to resist being divided. The parts of a fluid body are not at rest with respect to each other.[452]

What Descartes did *not* mean by "perfectly hard" was "perfectly elastic."[453] He clearly did *not* mean they were elastic in the historical sense that the body compressed and expanded like a spring, nor did he mean that they were perfectly elastic in the modern sense wherein the total kinetic energy is the same before and after the bodies meet. The "historical" notion of elasticity originated in the mid 17th century from the accounts of phenomena encountered initially in barometric experiments, such as those involving Torricellian tubes, as well as Mersenne and Jean Pecquet's later experiments and debates which described what would come to be known as the "spring of the air." Particularly influential was Jean Pecquet's *Experimenta nova anatomica* (1651). It used the terms *elasticus* and *elater*, which when the work was translated into English in 1653, were rendered as "elastick" and "spring."[454] Complicating matters, however, is the fact that Descartes's first rule of collision describes a scenario in which (if modern concepts of mass, velocity, and energy are used) kinetic energy would be conserved. After two equal masses with equal and opposite velocities meet in an elastic collision, they both subsequently move with velocities of the same magnitude but in opposite directions. This might lead one to assume that Descartes meant "elastic" by the term *durus.*[455] However, as we saw in chapter 3, Descartes's first rule is fairly unusual when compared to the other

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[452] AT VIII 70-1. *Principia* II 54, 55. "[T]hose bodies which are divided into very small parts which are agitated by a diversity of movements, are fluid; while those bodies whose particles are all contiguous and at rest, are solid." "[T]he parts of solid bodies are not joined by any other bond than their own rest." Translation by Miller, *Principles*, 70.
[453] Bertoloni Meli, *Thinking with Objects*, 229.
[454] Bertoloni Meli, *Thinking with Objects*, 225.
[455] This appears to be precisely what Valentine and Reese Miller did in their English translation of the *Principles of Philosophy*. They conflate the modern notion of perfectly elastic with Descartes notion of perfectly hard. See Miller, *Principles*, 64n.

six. Moreover, it is clear that the conservation of energy did not guide his first rule, nor was his notion of perfectly hard bodies equivalent to perfectly elastic bodies. Descartes's rules 5, 3 and 7a all describe hard bodies that move together and do not rebound after they meet, which conflicts with both historical and modern notions of elastic bodies. As has been shown in chapter 3, the key difference between Descartes's *early* and *later view* of collision is that in the latter (found in the *Principles*) Descartes stipulates the conditions in which the force of resistance is larger than the moving force, resulting in rebound rather than a transfer of motion.

In Beeckman's *Journal*,[456] in Descartes's early work on collision,[457] and in Borelli's rules of collision in *De vi percussionis* (1667),[458] all of which describe perfectly hard bodies, the bodies do not rebound after collision. Both Borelli and Beeckman realized that experience contradicted their mathematical accounts of the collision of perfectly hard bodies. Although Beeckman noted that atoms (perfectly hard bodies) do not rebound, at times he suggested that perhaps collections of atoms might somehow rebound, but he provided no rules for such collections.[459] Borelli seems to have considered the flexible and compressible nature of bodies from experience to be akin to other complicating factors, such as being irregular in shape, and did not provide mathematical rules of collision to accommodate such complications. Despite focusing his mathematical rules on hard bodies that do not rebound, Borelli also attempted an account of reflection, but was "at pains to explain whence reflection arises."[460]

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[456] See chapter 2
[457] See chapter 3
[458] Bertoloni Meli, *Thinking with Objects*, 229-31.
[459] See chapter 2.
[460] Bertoloni Meli, *Thinking with Objects*, 231.

The significant change between Descartes's early and late view is that in his later work, Descartes had provided a mathematical account of the conditions of rebound – namely when the force of resistance in a resting body cannot be overcome by a moving body. This he stipulated in rule 4: when a smaller body meets a larger body at rest, the smaller body does not move the larger and is repelled in the opposite direction. Although in Descartes's early view, he provided numerous quantitative examples in which smaller bodies move larger bodies at rest, Descartes changed his position to include rule 4 to explain whence reflection arises.[461] As Descartes explained to Clerselier, "without this [that which is described in rule 4], no body would ever be reflected by encountering another."[462]

Huygens did not rely on a Cartesian definition of hard bodies. Rather, hardness was an independent property.[463] And Huygens's hard bodies behaved differently from Descartes's (as well as Beeckman's and Borelli's). They behaved in a manner that others were at pains to explain (Beeckman and Borelli failed to explain rebound quantitatively, and Descartes appealed to the contest with the force of resistance). Nor did Huygens *explain* rebound as Descartes did from prior notions (or as Beeckman and Borelli did qualitatively). For Huygens, hard bodies rebound of their own nature.[464] He did not attempt to reconcile perfect hardness as an original quality of matter with (what would

[461] See chapter 3

[462] AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261. Also see Gabbey, "Force and inertia," 269. Gabbey provides a slightly different translation: "And unless that happens no body will ever be reflected by collision with another."

[463] Snelders, "Huygens and the concept of matter," 108, 110, 115, 121. In Huygens's *Discours sur la cause de la pesanteur*, which was appended to his *Traité de la Lumière* (1690), he claimed that hardness is an essential property of matter. Parts of the *Discourse* had been explained at the Académie Royale des Sciences in 1669. For the text of the *Discours sur la cause de la pesanteur* see HOC 21: 443-488.

[464] In a late manuscript dated to 1689, Huygens claims that hard nonelastic bodies rebound similarly to elastic bodies. But in a *perfectly* hard body, such as an atom, impact is instantaneous, whereas in an elastic body (which is not perfectly hard) impact occurs in a finite amount of time. See HOC 16: 210. Also see Bertoloni Meli, *Thinking with Objects*, 235.

come to be called) perfect elasticity, even while explicitly disagreeing with Leibniz on the relationship between absolute hardness and elasticity.[465] Although speculative, it is plausible that Huygens's link between hard bodies and rebound may have originated from his experiments with physically hard pendulum bobs and other experimental devices.

Even though Huygens used different notions of motion and body, he still worked within the general structure Descartes had proposed, and criticized Descartes's rules of collision non-empirically. Huygens's also seems to have been sympathetic to the prospects of "Cartesian" natural philosophical explanations in terms of the collision of moving particles.[466] In a manuscript from 1656 relating to his early work on collision he wrote:

> For if the whole of nature consists of certain particles, from the motion of which all the diversity of things arises, and by the extremely rapid impulse of which light is propagated and spreads through the immense spaces of the heavens in a moment of time, as many philosophers deem probable, this examination [of nature] will seem to be helped no small amount if the true laws by which motion is transferred from body to body be made known.[467]

However, Huygens did not seem to start by supposing absolutely hard atoms or particles, and to then develop a system of natural philosophy. Rather, Huygens seems to have initially approached the topic of collision as a problem in mechanics, which he investigated with physical objects such as pendulums. Huygens's tendency to avoid philosophical speculation and system building in preference for rigorous solutions to

[465] Snelders, "Huygens and the concept of matter." 115-121. Also see the set of letters between Huygens and Leibniz from 1692-94, which includes HOC 10: 296-304, Huygens to Leibniz, 11 July 1692, and HOC 10: 383-9, Huygens to Leibniz, 12 January 1693.
[466] Westman, "Huygens and the problem of Cartesianism," 83-103, 94. Westman provides an overview of various historiographical discussions of Huygens relationship to "Cartesianism." He also reproduces extensive passages from the notes Huygens made on a copy of Adrien Baillet's *Life of Descartes* (1691), which provide "Huygens' most direct comments on Descartes to be found anywhere in his extant writings."
[467] HOC 16: 150. Translation by Westfall, *Force in Newton's Physics*, 147.

particular problems has been noted by many. For example, Gabbey points this out, but with some additional criticism:

> Huygens' mechanical theory...lacks a serious examination of some of the critical problems caused by his partial adoption of the Cartesian mechanical model of the world. [Namely, he omits] a causal account of how one particle acts on another. Huygens does not tell us what force is, he provides no ontological account of what it is that causes one particle to change the speed or direction of another.[468]

Huygens did not over look these questions. He just did not consider them to be "critical problems," as is evidenced in letters with Henry Oldenburg. At the Royal Society William Neile had been engaging John Wallis with concerns regarding the determination of a causal account of how one body acts on another (*i.e.* discovering the physical cause of the communication of motion). Huygens addressed the topic directly:

> The question of whether motion is communicated to bodies by the elasticity of the parts, or by that of the air or of some other substance squeezed between them, or by hardness alone has not yet been resolved. But in the demonstration of my rules it does not matter at all which of the three one assumes.[469]

And a few months later in another letter: "As for Mr. Neile's question, what is the reason why a body puts another which it meets into motion, I do not think that this can be found out by any better known principles."[470]Although Mach also noted the "deficiency" in Huygens's "philosophical endowments," he ranked him as Galileo's peer, who continued the research that Galileo had begun, in a manner much like Galileo's.[471] And Mahoney has highlighted those in the 17th century who referred to him as an "Archimedes"—the genius who established mathematical principles to solve physical problems.[472]

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[468] Gabby, "Huygens and mechanics," 175-6.
[469] OCH 6:161-5. Huygens to Oldenburg, 31 July 1669.
[470] OCH 6: 289-92. Huygens to Oldenburg, 20 October 1669.
[471] Ernst Mach, *The Science of Mechanics: a critical and historical account of its development* (Chicago: Open Court, 1893), 155-7. The first edition was published in 1883.
[472] Mahoney, "Measurement of time and longitude," 234.

Once Huygens had formulated general principles and organized them into an axiomatic treatise (*De motu corporum ex percussione*), the heuristic paths, such as the algebraic equations and the experiments, become secondary or completely absent in the formal axiomatic presentation.[473] Then, with general principles in hand or at least in sight, acquired through the solution of specific problems, Huygens seems to have suggested, as he did in the passage quoted above that, "the laws by which motion is transferred from body to body" may help examinations of nature which suppose that nature consists of particles in motion. In addition to Huygens's non-Cartesian understanding of matter and motion, Huygens Galilean/Archimedean axiomatic formulation is in sharp contrast to that of Descartes.

Section 5
Conclusion

Huygens's ideas on collision were first developed with his innovative use of symbolic algebra and his expert understanding of the pendulum. They were then presented and justified in the axiomatic tradition of Archimedes and Galileo.

Huygens used Cartesian symbolic algebra as a tool for conceptual analysis. He not only represented Descartes's principle of the conservation of quantity of motion and his rules of collision algebraically, but in doing so Huygens criticized rules 4 and 5 and could show that Cartesian quantity of motion is not conserved. This heuristic work corresponds to Huygens's first announcements of his rejection of Descartes's rules of collision.

[473] Bertoloni Meli, *Thinking with Objects*, 233. I have slightly modified what Bertoloni Meli calls a "Huygensian style," which was "inspired by Archimedes, in which the search for general principles and axioms takes center stage and experience plays a secondary role as part of the scaffolding rather than part of the formal presentation."

Huygens's use of algebra—the representation of multiple kinds of physical quantities in a single equation—was new. Thomas Harriot had previously and independently used symbolic equations in *De reflexione corporum rotundorum*, but as we saw in chapter 2, they were symbolic representations of the steps of a geometric construction. Huygens's innovative algebraic study of collision remained in his private manuscripts. In the treatises prepared for publication Huygens quantified motion in the tradition of Galileo, and in publications such as the *Horologium oscilatorium*, he rigorously used the classical theory of proportions. The algebra in the manuscripts bears some historical peculiarities. It is not a purely formal system. The symbols refer to specific quantities and are bound by the restraints such quantities impose, and Huygens actively avoided producing negative results. His investigations of collision with equations, particularly his engagement with negatives and algebraic operations, may have also served as scaffolding for a new principle, the conservation of Cartesian quantity of motion with direction—quantity of motion is conserved if motion in a contrary direction is subtracted.

In addition to the critical conceptual analysis of Descartes's theory of collision, and the scaffolding for a new principle, Huygens algebraic equations successfully predicted the outcomes of experiments with colliding pendulum bobs at the Royal Society. Huygens had worked extensively on the properties of the pendulum. It was not only important for the empirical investigation of collision, but the pendulum was also key for the formation of several principles of his theory of collision, such as his variant of the Torricelli principle, the reversibility of impact, and perhaps even the conservation of "body times speed squared."

Although Huygens's interest in collision was inspired by Descartes, he presented and justified his theory of collision with an axiomatic formulation in the tradition of Archimedes and Galileo. Three of Descartes's rules (6, 7c, and 1) had been guided by implicit notions of symmetry, as we saw in chapter 3 section 5.4.3. Huygens's axioms ("hypotheses"), and the key to his argument, relied on principles of symmetry: he used the symmetrical case of collision (similar to Descartes's first rule), in conjunction with the relativity of motion, to show the equivalence between different scenarios of collision.

Huygens appears to mount his criticisms from within Descartes's system, however in doing so he re-conceptualized the two fundamental components of the system, matter and motion. With Huygens's accurate predictions of colliding pendulum bobs, Huygens could have criticized Descartes's rules for conflicting with experience. Instead, in his early manuscripts Huygens used Descartes's own symbolic algebra against Descartes's rules of collision. And, throughout his work, Huygens used Descartes's 1st rule and relative motion (which bears some resemblance to Descartes's notion of motion) against the Cartesian rules. Huygens's shared in Descartes's hope that all natural phenomena could be explained in terms of matter and motion. However, in his reformulation, Huygens did not share a Cartesian understanding of those concepts. Huygens espoused a radical notion of relative motion in which no perspective is privileged, rather than Descartes's notion of motion, which in its "proper" conception defines true motion to be the transference of a body with respect to its immediate neighborhood. Huygens's perfectly hard bodies rebound perfectly, unlike Descartes's hard bodies, for which Descartes provided a mathematical explanation of the conditions of rebound rooted in the contest between the moving force and the force of resistance.

With these new notions, and Descartes's mathematical tools, Huygens opened new horizons in the algebraic study of nature, and challenged the basic rules of Descartes's physics. Two of the individuals present when Huygens performed his predictions for the Royal Society in 1661, John Wallis and Christopher Wren, went on to publish their own rules of motion in terms of algebraic equations in 1669. As we will see in the next chapter, the work of Wallis and Wren shows the legacy of Huygens's pendulum in the experiments at the Royal Society, as well as the importance of symmetry, and the continued transformation of the mathematics of nature, particularly in the critical role of positive and negative signs.

Chapter 5
The mathematics of collision in the Royal Society: Experiments, the balance, and the algebraic language of nature

Dr. Wren produc'd before the Society, an Instrument to represent the effects of all sorts of Impulses, made between two globous Bodies, either of equal, or of different bigness, and swiftness, following or meeting each other, or the one moving, the other at rest. From these varieties arose many unexpected effects of all which he demonstrated the Theories, after they had been confirm'd by many hundreds of Experiments in that Instrument. These he propos'd as the Principles of all Demonstrations in Natural Philosophy: Nor can it seem strange, that these Elements should be of such Universal use; if we consider that Generation, Corruption, Alteration, and all the Vicissitudes of Nature, are nothing else but the effects arising from the meeting of little Bodies, of differing Figures, Magnitudes, and Velocities.

—from Thomas Sprat's *History of the Royal-Society of London* (1667)

...to know a thing barely by experiment is good for use but it is not science or philosophye.

—William Neile to Henry Oldenburg, 7 May 1669

Now what is admitted in Lines, must on the same Reason, be allowed in Plains also.

As for instance: Supposing that in one Place, we Gain from the Sea, 30 *Acres, but Lose in another Place,* 20 *Acres: If it be now asked, How many Acres we have gained upon the whole: The Answer is,* 10 *Acres, or* $+10$. *(Because of* $30-20=10$*). Or, which is all one* 1600 *Square Perches.* [...] *Which if it lye in a Square Form, the Side of that Square will be* 40 *Perches in length* [...]

But if then in a Third place, we lose 20 *Acres more, and the same Question be again asked, How much we have gained in the whole; the Answer must be* -10 *Acres. (Because* $30-20-20=-10$.*) That is to say The Gain is* 10 *Acres less than nothing. Which is the same as to say, there is a Loss of* 10 *Acres: or of* 1600 *Square Perches.*

And hitherto, there is no new Difficulty arising, nor any other Impossibility than what we met with before, (in supposing a Negative Quantity, or somewhat Less than nothing:) Save only that $\sqrt{1600}$ *is ambiguous; and may be* $+40$, *or* -40.

We cannot say it is 40, *nor that it is* -40. *(Because either of these Multiplyed into itself, will make* $+1600$*; not* -1600.*)*

But thus rather, that it is $\sqrt{-1600}$, *(the Supposed Root of a Negative Square;) or (which is Equivalent thereunto)* $10\sqrt{-16}$, *or* $20\sqrt{-4}$, *or* $40\sqrt{-1}$.

—from John Wallis's *Treatise of Algebra* (1685)

CHAPTER 5 OUTLINE

Section 1
Introduction

On a Thursday meeting of the Royal Society in October of 1668, "it was proposed by some [including Robert Hooke[474]], that[475] there might be made some experiments to discover the nature & laws of motion, as the foundation of Philosophie and all Philosophical discourse."[476] Other items discussed at the October meeting included an eye witness account of a unicorn,[477] Regnier de Graaf's *De virorum organis generationi inservientibus de clysteribus et de usu siphonis in anatomia* (1668),[478] and the earl of Sandwich's observations of a comet and lunar eclipse while in Spain and Portugal. Discussion of John Wilkins's newly published work, *An Essay towards a Real Character and a Philosophical Language*, was put off until the next meeting.[479]

On that Thursday in October, members of the Royal Society recalled that Christopher Wren and Christiaan Huygens had already "considered that subject [the nature & laws of motion] more than many others, & probably found out a Theory to explicate all sorts of experiments to be made of that nature."[480]

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[474] Thomas Birch, *The History of the Royal Society of London*, vol. 2 (London, A. Millar in the Strand, 1756) 315. Robert Hooke had proposed that these experiments should be prosecuted. "... it being proposed by Mr. Hooke, that the experiments of motion might be prosecuted, thereby to state at last the nature and laws of motion..."
[475] I have expanded the contractions "yt" (that), "ye" (the), and "yn" (than) in the 17th century English texts, which have been reproduced in the OCH.
[476] OCH 5: 117. Oldenburg to Wren, 29 October 1668.
[477] Birch, *History* 2: 315. "Mr. Oldenburg produced the papers brought by Sir Robert Southwell from Portugal, written by Father Jerom Lobo, a Jesuit, and an eye-witness of most of the particulars contained therein, which were, 1. a relation of the river Nile, its source, current, and inundation. 2. An account of the real existence and the place of abode of the unicorn. 3. Of the Abyssine emperor, vulgarly called Prester John. 4. Of the Red-Sea and the cause of its denomination. 5. A discourse of palm-trees, their variety, fruit, usefulness, proper soil, &c." Jerome Lobo's account of his travels would later be translated into French by Abbe Legrand in 1728, and from French into English by Samuel Johnson in 1735. See Father Jerome Lobo, *A Voyage to Abyssinia*, trans. Samuel Johnson, ed. Henry Morley (London: Cassell & Co., 1887).
[478] Birch, *History* 2: 315.
[479] Ibid.
[480] OCH 5: 118. Oldenburg to Wren, 29 October 1668. Birch, *History* 2: 315. According to Birch's *History* the president of the Royal Society may have thought it redundant to prosecute such experiments: "After this [the proposal that experiments on motion might be prosecuted to determine the nature and laws of motion]

Huygens had by that time already made a name for himself in the international scholarly community,[481] and had visited London in 1661 where—using algebraic calculations—he correctly predicted the results of experiments concerning impact, as we have seen in the previous chapter.[482] His work on collision dates to 1652.[483] By 1656 Huygens had essentially completed his treatise, *De motu corporum ex percussione*, which would not be published until 1703, after his death.[484] A summary of this treatise would be published in the *Journal des Sçavans* as well as the *Philosophical Transactions* in 1669.[485] Christopher Wren had been active in the meetings at Gresham College in London prior to the existence of the Royal Society. In fact, beginning about 1658 meetings were held after Wren's Wednesday or Thursday lectures as Professor of

... the president desired, that it might be considered, whether it were so proper or necessary to try this sort of experiments, since Monsr. Huygens and Dr. Christopher Wren had already taken great pains to examine that subject, and were thought to have also found a theory to explicate all the phœnomena of motion."

[481] H. J. M. Bos, "Christiaan Huygens: a biographical sketch," *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980), 7. Bos calls 1656 "a turning-point in Huygens' career." Huygens invented the pendulum clock, obtained a patent and published a short book on it. He published on the ring of Saturn in 1659. "By 1660 he was famous."

[482] When Huygens responded to the Royal Society's request to share his work on motion, Huygens first asked a clarifying question: "I beg you therefore, Sir, to let me know what part of motion they wish me to treat first, for there are several sorts, as you know, most of which I think I have considered: that is, the ratio of the fall of heavy bodies, both with and without the resistance of air; the motion of pendulums; centers of oscillation; circular and conical motion, and centrifugal force; the communication of motion by impact..." Concerning the latter he recalled the 1661 visit and experiments in London: "...I remember Mr Wren and Mr. Rooke showed me their experiments when I was in England, and they agreed very well with what I determined on the spot should be the case according to my hypotheses." See OCH 5: 127. Huygens to Oldenburg, 3 November 1668. Translation by Hall & Hall.

[483] HOC 16: 92.

[484] HOC 16: 30-91.

[485] The former, recorded on 18 March 1669, was published in the *Journal des sçavans* 2 (1667-71): 531-6. It has also been collected in HOC 16: 179-81, (also see HOC 6: 383-5, Huygens to Gallois, 18 March 1669). The title in the *Journal des sçavans* is "Extrait d'une Lettre... " However, Huygens appears to have given the document the following title: "Regles du mouvement dans la rencontre des Corps." The latter summary of the treatise, recorded as 12 April 1669, was published in the *Philosophical Transactions* 4 (1669): 925-8. It has also been collected in HOC 6: 429-33. The title in the *Philosophical Transactions* is "A Summary Account of the Laws of Motion..." However, Huygens appears to have given the document the following title: "Regulæ de Motu Corporum ex mutuo impulsu." Note that Huygens consistently referred to the document "rules of motion," rather than "laws of motion," as the editor of the *Philosophical Transactions* did. The version printed in the *Journal des sçavans* is slightly different from the paper originally submitted to the Royal Society. See A. Rupert Hall, "Mechanics and the Royal Society, 1668-70," *The British Journal for the History of Science* 3 (1966): 33-5. Huygens also sent demonstrations for several of his proposition to the Royal Society on 5 January 1668/9, which were not published in the *Philosophical Transactions.* See HOC 6: 336-43.

Astronomy at Gresham.[486] Wren was present when Huygens visited London in 1661 to predict the motion of pendulum bobs upon impact using his private algebraic calculations. Prior to this, in the years just after the civil war, while studying at Wadham College, Oxford, Wren met with the "virtuous and learned Men, of Philosophical Minds" who gathered at John Wilkins's lodging at Wadham-College.[487] These men—Royalists and Parliamentarians alike—would go on to meet at Gresham and subsequently form the Royal Society after the "restoration of the monarchy." Wren had joined Wadham the same year that Charles I was executed and the monarchy was abolished. After the "return of Charles II," Wren was instrumental in the official formation of the Royal Society—writing the preamble, for example, for the charter of incorporation, which marked its official formation.[488]

After the meeting in October 1668, Henry Oldenburg, the secretary of the Society, was directed to ask Wren, "as well as Monsr Hugens, in the name of the Society, [if they]

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[486] Thomas Sprat, *The History of the Royal Society of London* (London: T. R. for J. Martyn and J. Allestry, 1667) 57-8. Also see Christopher Wren, *Parentalia* (London: T. Osborn and R. Dodsley, 1750), 196. The following is quoted from the *Parentalia*, which differs slightly from the original passage in Sprat's *History*. "...being called away to several Parts of the Nation, and the greatest Number of them coming to *London*, they usually met at *Gresham-College*, at the *Wednesday's* and *Thursday's* Lectures of Dr. Wren (Professor of *Astronomy*) and Mr. *Rook*, (Professor of Geometry.) This Custom was observed once if not twice a Week, in Term-Time; 'till they were scattered by the miserable Distractions of that fatal Year, when the Continuance of their Meetings there might have made them run the Hazard of the Fate of *Archimedes*: For then the Place of their Meetings was made a *Quarter for Soldiers*."

[487] Sprat, *History*, 53. Also see Wren, *Parentalia*, 196. The following is quoted from the *Parentalia*, which differs slightly from the original passage in Sprat's *History.* "Some Space after the Conclusion of the Civil Wars, Dr. *Wilkins*'s Lodging at *Wadham-College* in *Oxford*, was made the Place of Resort for virtuous and learned Men, of Philosophical Minds, where the first Meetings were held which laid the Foundation of the *Royal Society* for improving of natural Knowledge: The principal and most constant at the Assemblies were Dr. *Seth Ward*, the Bishop of *Exeter*, Mr. *Boyle*, Dr. *Wilkins*, Dr. *Wallis*, Dr. *Willis*, Sir *William Petty*, Mr. *Matthew Wren*, Dr. *Godard*, Dr. *Bathurst,* Dr. *Christopher Wren*, and Mr. *Rook.* Here they continued without any great Intermissions, till about the Year 1658..."

[488] J. F. Scott, "Wren, Christopher," *DSB* 14: 509-11. "The charter of incorporation passed the great seal on 15 July 1662 (which thus is the date of the formation of the Royal Society); Wren is said to have prepared is preamble." For the text of the preamble, see Wren, *Parentalia*, 196-7. Sprat, however, set 1660 as the date the Royal Society began: "But upon the *Restoration* of the *King,* Philosophy had its Share in the Benefits of that glorious Action: For the *Royal Society* had its Beginning in the wonderful pacifick Year 1660, and as it began in that Time, when the *Kingdom* was freed from Confusion and Slavery; so in its Progress, its chief Aim hath been to redeem the Minds of Men from Obscurity, Uncertainty, and Bondage." See Sprat, *History*, 58. Quoted from Wren, *Parentalia*, 196.

would pleas to impart unto them what [they] had meditated & tryed on the said argument," assuring them that whatever they communicated would be registered by the Society and "stand in their booke as one of the best monuments of [their] Philosophicall Genius."[489] Wren presented his theory two months later at the 17 December meeting of the Royal Society.[490] The next month it was printed in the *Philosophical Transactions* with the title, *Lex naturae de collisione corporum* ["The Law of Nature in the Collision of Bodies"].[491] "A Summary Account by Dr. John Wallis, of the General Laws of Motion," which was communicated to the Society in November, was published with Wren's account in the same volume of the *Philosophical Transactions*.[492] Huygens's *Regles du mouuement dans la rencontre des corps* was printed in the *Journal des Sçavans* in March of 1669,[493] and a Latin translation, *Regulae de motu corporum ex mutuo impulsu*, was printed in the *Philosophical Transactions* in April of 1669.[494]

These texts were many years in the making and were the product of several experiments prosecuted by members of the Royal Society on instruments contrived for

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[489] OCH 5: 118. Oldenburg to Wren, 29 October 1668.

[490] OCH 5: 321n. "Wren himself produced his theory at the Society's meeting on 17 December 1668."

[491] Christopher Wren, "Lex naturae de collisione corporum," *Philosophical Transactions* 3 (1669): 867-8. It has also been collected in OCH 5: 319-20, with an English translation by Hall, OCH 5: 320-1. The title the editor of the *Philosophical Transactions* gave to the document is the following: "Dr. Christopher Wrens Theory concerning the same subject; imparted to the R. Society Decemb. 17 last, though entertain'd by the Author divers years ago, and verifie'd by many Experiments, made by Himself and that other excellent Mathematician M. Rook before the said Society, as is attested by many Worthy Members of that Illustrious Body."

[492] John Wallis, "A Summary Account of the General Laws of Motion," *Philosophical Transactions* 3 (1669): 864-6. It has also been collected in OCH 5: 164-7. Wallis to Oldenburg, 15 November 1668. Hall provided an English translation, OCH 5: 167-70. The title the editor of the *Philosophical Transactions* gave to the document is the following: "A Summary Account given by Dr. John Wallis, of the General Laws of Motion, by way of Letter written by him to the Publisher, and communicated to the R. Society, November 26, 1668."

[493] Christiaan Huygens, "Regles du mouuement dans la rencontre des corps," *Journal des sçavans* 2 (1667-71): 531-6. It is also collected in HOC 16: 179-81 (also see HOC 6: 383-5, Huygens to Gallois, 18 March 1669). The version in the *Journal des sçavans* was slightly different from the paper submitted to the Royal Society. See footnote above as well as Hall, "Mechanics and the Royal Society," 33-35.

[494] Christiaan Huygens, "Regulæ de Motu Corporum ex mutuo impulsu," *Philosophical Transactions* 4 (1669): 925-8. It is also collected in HOC 6: 429-33.

the purpose of studying collision. They are also the products of distinctive traditions of mathematics. Huygens's heuristic algebraic work (found in his manuscripts from the early 1650s) was derived from Descartes's *La Géométrie*, and Huygens's finished work on collision was in the classical mode of Archimedes and Galileo's *Discorsi*. Wallis's and especially Wren's theories, as will be argued below, stem from the English traditions of practical mathematics as well as the tradition of symbolic algebra derived in part from William Oughtred's *Clavis mathematicae*. The exchange between Huygens and Wren, who had different visions of the proper mathematical expression of theories, as well as differing attitudes regarding the relationship between mathematics and experiment, marks a transition in the mathematization of nature. Unlike Huygens's and Wren's predecessor's theories of collision, theirs were quantitative in ways that admitted of predictions and measurements. In addition to this new quantitative relationship to experiments, the theories themselves are structured mathematically, relying on notions of symmetry. Wren and Wallis expressed their theories in symbolic algebra, using positive and negative signs to indicate the directionality of motion. Wren's theory relies on notions of brevity and the economy of the symbolic algebra. As we will see, the algebraic equations were not merely a practical calculating device, but was meant to lay bare, "at a glance," the mysteries of the book of nature.

Section 2
Experiments on Collision

Hooke's proposal in 1668 was not the first time that someone in the Royal Society had suggested (or performed) experiments to determine the "laws of motion."[495] Experiments were performed in 1661 as well as 1666. The result of the activity in 1666 culminated in much the same way as Hooke's proposal in 1668—a decision to consult what had already been done on the topic, particularly Wren's prior work. However, it is apparent that the results of the experiments prosecuted in 1666 may well have been interpreted as a challenge to the Cartesian rules of collision, and almost certainly provoked questions regarding the nature of motion, and principles of conservation.

"The Great Fire of London" (September 2-5, 1666) had burned hundreds of acres of the city and destroyed, on some estimates, 13,200 houses and 84 churches, including St. Paul's Cathedral,[496] which Wren would later be responsible for rebuilding, as well as perhaps 52 other churches.[497] A month after the fire, "an experiment was tried of the propagation of motion by a contrivance,

> whereby two balls of the same wood, and of equal bigness, were so suspended, that one of them being let fall from a certain hight [sic] against the other, the other was impelled upwards to near the same hight, from which the first was let fall, the first becoming then almost quiescent, and the other returning, impelled the first

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[495] Hall, "Mechanics and the Royal Society," 27. "There can be no doubt that when the 'laws of motion' are spoken of at this time the words were used in a wholly Cartesian sense to signify the law of inertia as the foundation of all, then the laws of the distribution of motion among colliding bodies." Also see instances in the records of the Royal Society (reported by Birch) in which references to the experiments regarding the propagation or communication of motion in collision and "laws of motion" are used interchangeably: Birch, *History* 2: 140 (January 16, 1666/7), 315 (October 22, 1668), 320 (November 12, 1668), 328 (November 26, 1668), 344-345 (February 4, 1668/9), 347 (February 18, 1668/9), and 392 (July 1, 1669). Steinle also notes that "the discussion of the collision of bodies was the first in which the talk of laws was broadly used, first in England, but soon all over Europe. See Friedrich Steinle, "From Principles to Regularities: Tracing 'Laws of Nature' in Early Modern France and England," in *Natural Law and Laws of Nature in Early Modern Europe*, ed. Lorraine Daston and Michael Stolleis (Burlington: Ashgate, 2008), 221.

[496] Bruce Robinson, "London's Burning: The Great Fire," last modified March 29, 2011, http://www.bbc.co.uk/history/british/civil_war_revolution/great_fire_01.shtml.

[497] Wren, *Parentalia*, 263. J. F. Scott, *The Mathematical Work of John Wallis, D.D., F.R.S. (1616-1703)* (London: Taylor and Francis, LTD., 1938), 216.

> upwards again to almost the same hight it had fallen from before, itself becoming then in a manner motionless, till after some returns they both vibrated together. It was ordered, that this experiment be prosecuted, and others of that kind thought upon.[498]

This experiment seems to conflict with Descartes's 6th rule, at least when the Cartesian rule is *loosely* interpreted. Remember that Descartes's rules of collision were not designed to directly explain the impact of wooden pendulum bobs, and Descartes himself acknowledged that his rules conflicted with objects from experience. Nevertheless, there seems to have been some confusion regarding the domain of Descartes's rules. Thomas Sprat in his *History of the Royal* Society (1667), for example, appears to have thought of (or at least presented) Descartes's rules as based upon crude experiments with tennis and billiard balls.[499] Descartes's 6th rule stated that

> "if body C were at rest, and exactly equal in size to body B, which was moving toward it, then it would be necessary that it would in part be impelled by B, and in part it will make it rebound, so that if B went toward C with four degrees of speed, it would be necessary that it transfer one to it, and with the three remaining [degrees] would return in the direction from which it had come."[500]

Descartes, of course, had well-formed reasons for thinking this, which have to do with the contest between the moving force and the force of resistance.[501]

In addition to "thinking upon" the results of these experiments, during the following week's meeting "the experiment about propagating [sic] of motion was prosecuted with three balls."[502] This was an expansion of the previous experiment on the

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[498] Birch, *History* 2: 116-7. October 17, 1666.
[499] Sprat, *History*, 312.
[500] AT IX 92. *Principes* II 51. Translation by Daniel Garber, *Descartes' Metaphysical Physics* (Chicago: Chicago University Press, 1992), 259.
[501] See chapter 3.
[502] Concerning the Society's "directing" of experiments, Sprat wrote in his *History* that "it has been their usual course, when they themselves appointed the *Trial*, to propose one week, some particular *Experiments,* to be prosecuted the next; and to debate beforehand, concerning all things that might conduce to the better carrying them on." See Sprat, *History*, 95. Birch is very consistent in his *History* to distinguish between "trying" and "prosecuting" an experiment.

contrivance with only two wooden balls. The middle ball in the new experiment remained at rest even upon being "struck by either of the lateral ones, which impelled each other upwards."[503] One of the topics that members of the Society "thought upon" while prosecuting these experiments was whether or not motion could be created and destroyed. Members seem to have thought that the experiments, such as those described above, make it apparent that motion does not "die." When prosecuted again in 1668, the members of the Royal Society continued to consider the best means to determine whether or not *motion* decreases or remains constant. In fact in November of 1668, "Mr. Hooke was ordered to think upon other experiments for the making out this hypothesis about motion, which is, that no motion dies, nor is any motion produced anew."[504]

The hypothesis would be debated with enthusiasm among members of the Royal Society, as evidenced, for example, in the lively correspondence between William Neile and John Wallis (the latter arguing that motion can be destroyed),[505] as well as Willughby's and Croone's criticisms of both Wren's and Huygens's published theories of motion in the *Philosophical Transactions*.[506] Huygens, for instance, consistently wrote that when two equal bodies meet moving at the same speed, they rebound with the *same*

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[503] Birch, *History* 2: 117. October 24, 1666. Nearly the same experiment was prosecuted again two years later on 12 November 1668. In 1668 the relationship between hardness, "springiness," and rebound were specifically investigated using this contrivance. See Birch, *History* 2: 320. November 12, 1668. Such phenomena are familiar to most modern readers who have encountered "Newton's Cradle" (also known as an "Executive Ball Clicker")—a toy usually made of five metal balls suspended in line with each other by wires.

[504] Birch, *History* 2: 320. November 12, 1668.

[505] OCH 5: 263-5 (Neile to Oldenburg, 18 December 1668), 272-5 (Wallis to Oldenburgh, 21 December 1668), 286-7 (Neile to Oldenburg, 28 December 1668), 302-4 (Wallis to Oldenburg, 2 January 1668/9), 312-4 (Neile to Oldenburg, 2 January 1668/9), 336-8 (Wallis to Oldenburg, 12 January 1668/9), 346-7 (Neile to Oldenburg, 22 January 1668/9), 363-4 (Neile to Oldenburg late January 1668/9), 517-8 (Neile to Oldenburg, 7 May 1669), 540-2 (Wallis to Oldenburg 10 May 1669), 550-1 (Wallis to Oldenburg, 17 May 1669), 558-9 (Neile to Oldenburg, 20 May 1669), 573-4 (Wallis to Oldenburg, 29 May 1669).

[506] Hall, "Mechanics and the Royal Society," 35-37. Hall provides an overview of the concerns of Croone, Willughby, and Neile. Also see Dana Jalobeanu, "The Cartesians of the Royal Society," in *Vanishing Matter and the Laws of Motion: Descartes and Beyond*, eds. Dana Jalobeanu and Peter R. Anstey (New York: Routledge, 2011), 103-129, as well as Scott, *Mathematical Work of Wallis*, 104-6.

speed with which they approached. He does not write that they rebound with speeds equal in magnitude to their original speeds. He writes that it is the same speed. Impact is instantaneous. Motion is not interrupted. The direction merely changes. And, according to Huygens, a change of direction did not mean a change in speed.[507] The topic becomes complex when collision is understood algebraically rather than geometrically.

As we have seen in the previous chapters, there was widespread disagreement on not only the answers given to the question of whether motion remains constant, but there was disagreement on what phenomenon or "physical quantity" was even under investigation with the question. Isaac Beeckman had been compelled by his mathematical investigations of collision to conclude that *motion* itself must in fact decrease and die, but God—operating contrary to what Beeckman's mathematical argument showed—continually "enlivens" motion. Descartes, on the other hand, set as a fundamental principle that the *quantity of motion* (which was proportional to both body and motion) is conserved. Contrary to Descartes, Huygens argued that the *Cartesian quantity of motion* can increase or decrease, and, relying on the notion of the relativity of motion (which Descartes would not have accepted in this form[508]), he persuasively demonstrated this.[509]

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[507] HOC 16: 30-1. "Hypothesis II: Whatever may be the cause of hard bodies rebounding from mutual contact when they collide with one another, let us suppose that when two bodies, equal to each other and having equal speed, directly collide with one another, each rebounds with the same speed which it had before the collision." Translation by Richard J. Blackwell, "Christiaan Huygens' *The Motion of Colliding Bodies*," *Isis* 68 (1977): 574. Also see Richard Westfall, *Force in Newton's Physics: The Science of Dynamics in the Seventeenth Century* (New York: American Elsevier, 1971), 156.

[508] Although there has been continued disagreement on the correct interpretation of Descartes's conceptions of motion, most commentators would agree that Descartes's account of "relative motion" is not the same as Huygens's. See chapter 4. Nevertheless, Huygens's contemporary, John Wallis, seems to have conflated the two. OCH 6: 189. Wallis to Oldenburg, 15 August 1669: "'Tis very little I have to say in answere to yours of Aug. 4 [...] nor to the note inclosed concerning M. Huygens, with whom I do for the most part concur, though not in all. For I am not yet satisfyed in that notion of Descartes, which hee seems to imbrace, that Motion is onely relative; & , of the two bodies separated, it is indifferent whether of the two be sayd to move."

[509] HOC 16: 49. In Proposition 6 of *De motu corporum ex percussione* Huygens demonstrates the following: "When two bodies collide with one another, the same quantity of motion in both taken together

This appears to have been the quantity that concerned Willughby. He seems to have thought that the notion that it could be destroyed or "created from nothing" defied common sense—it was "absurd" and "incredible."[510] However, as Huygens would also claim, if direction is considered (*i.e*. if motion in a contrary direction is *subtracted*), then the "quantity of motion" would be conserved.[511] Adding more complexity to the topic, Huygens also introduced a new principle—the conservation of the product of body and the square of its speed (later known as the *vis viva*), but this principle was not valid for every kind of body. In other words, although the Royal Society was in the midst of considering whether or not motion dies and is created anew, it was not clear to all which quantity should be investigated as being conserved. Instead of motion, it may be the Cartesian quantity of motion, the quantity of motion with direction, or the product of body and the square of its speed.

Upon the proposal of Robert Moray, in the months following the trial and prosecution of these experiments in the autumn of 1666, the Society discussed "how the experiments at the public meetings of the society might be best carried on; whether by a continued series of experiments, taking in collateral ones, as they were offered, or by going on in that *promiscuous way*, which had hitherto obtained."[512] Of particular concern was whether or not the society should continue prosecuting "the experiments for

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does not always remain after impulse what it was before, but can be either increased or decreased." Translation by Michael Mahoney, "Christiaan Huygens: On the motion of bodies resulting from impact," accessed January 25, 2015, http://www.princeton.edu/~hos/Mahoney/texts/huygens/impact/huyimpct.html.

[510] Hall, "Mechanics and the Royal Society," 35.

[511] HOC 6: 429-33. Huygens, "Regulæ de Motu Corporum ex mutuo impulsu," 925-8. Huygens includes this claim of the conservation of quantity of motion with direction in the paper published with the *Philosophical* Transactions, which also states that the Cartesian quantity of motion can increase, decrease, or remain the same. *Quantitas motus duorum Corporum augeri minuive potest per eorum occursum; at semper ibi remanet eadem quantitas versus eandem partem, ablata inde quantitate motus contrarii.* "The quantity of motion which two hard bodies have may be increased or diminished by their collision, but when the quantity of motion in the opposite direction has been subtracted there remains always the same quantity in the same direction." Translation by Hall, "Mechanics and the Royal Society, " 34.

[512] Birch, *History* 2: 131-2. December 4, 1666. Italicized emphasis added.

propagating motion, and the magnetic ones," since Huygens and Balle respectively had already "engaged themselves particularly" on these topics.[513] Oldenburg was directed to consult the *Journal*, and in a meeting in the following month, it was decided by the council that the experiments "for making out a theory of the laws of motion formerly begun by Wren, Dr. Croune, and Mr. Hooke...should be prosecuted." The Society also asked Wren "to give in those experiments of motion devised by himself." However, at the time Wren alleged that the account of his experiments had been left at Oxford.[514] However, the variety of experiments at the meetings presumably continued "going on in that promiscuous way" (as Moray had put it), and the experiments on the propagation of motion would not be mentioned again until Hooke's proposal at a meeting almost two years later (October 1668).

[513] Ibid.
[514] Birch, *History* 2: 140. January 16, 1666/7.

2.1 – Wren's authority:
The Doctrine of Motion "confirm'd by many hundreds of experiments"

A decision was made to consult Christopher Wren's prior work on collision after both the Royal Society's sets of experiments with the contrivance of suspended wooden balls in 1666 and Hooke's 1668 proposal that "there might be made some experiments to discover the nature & laws of motion, as the foundation of Philosophie and all Philosophical discourse."[515] Wren's authority on the topic was also emphasized in the *History of the Royal Society* (1667). In the *History* Sprat attributed to Wren—as Wren's "primary achievement" "to which he may lay peculiar claim"—"the *Doctrine of Motion*, which is the most considerable of all others, for establishing the first *Principles of Philosophy*."[516] In doing this Sprat broke with the general plan in his *History*, which refrained from discussing particular individuals in the Society.[517] Sprat appears to have thought that Wren's achievements had not been sufficiently recognized, and so he singled him out to undo this injustice. According to Sprat, they had been "casually omitted" from the register of the society,[518] and in addition, perhaps, had not been recognized previously because of Wren's own modesty.[519]

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515 OCH 5: 117. Oldenburg to Wren, 29 October 1668. Birch, *History* 2: 315.
516 Sprat, *History*, 312.
517 Ibid., 311. "In the whole progress of this *Narration*, I have been cautious to forbear commending the labours of any Private *Fellow* of the *Society*. [...] But now I must break this *Law*, in the particular case of Dr. *Christopher Wren.*"
518 Ibid., 311. "But I only do it on the meer consideration of Justice: For in turning over the *Registers* of the *Society*, I perceived that many excellent things, whose first *Invention* ought to be ascrib'd to him, were casually ommitted: This moves me to do him right by himself, and to give this separate Account of his indeavours, in promoting the Design of the *Royal Society*, in the small time wherein he has had the opportunity of attending it."
519 Ibid., 317-8. "This is a short account of the principal Discoveries which Dr. *Wren* has presented or suggested to this *Assembly.* [...] [I]t is reasonable, that the original *Invention* should be ascrib'd to the true *Author*, rather than the *Finishers.* Nor do I fear that this will be thought too much, which I have said concerning him: For there is a peculiar reverence due to so much excellence, cover'd with so much modesty. And it is not Flattery but Honesty, to give him his just praise; who is so far from usurping the fame of other men, that he endeavours with all care to conceal his own."

Sprat placed particular emphasis on the role of instruments and experiments to establish the *Doctrine of Motion*:

> This *Des Cartes* had before begun, having taken up some *Experiments* of this kind upon Conjecture, and made them the first *Foundation* of his whole *System of Nature*: But some of his Conclusions seeming very questionable, because they were only deriv'd from the gross Trials of Balls meeting one another at *Tennis*, and *Billiards*: Dr. *Wren* produc'd before the *Society*, an *Instrument* to represent the effects of all sorts of *Impulses,* made between two globous Bodies, either of equal, or of different bigness, and swiftness, following or meeting each other, or the one moving, the other at rest. From these varieties arose many unexpected effects of all which he demonstrated the *Theories*, after they had been confirm'd by many hundreds of *Experiments* in that *Instrument*. These he propos'd as the Principles of all *Demonstrations* in *Natural Philosophy*: Nor can it seem strange, that these *Elements* should be of such Universal use; if we consider that *Generation, Corruption, Alteration,* and all the Vicissitudes of *Nature*, are nothing else but the effects arising from the meeting of little Bodies, of differing Figures, Magnitudes, and Velocities."[520]

Even Descartes's rules are presented in terms of experiments, albeit unsatisfactory experiments: "gross Trials of Balls meeting one another at *Tennis,* and *Billiards*." As we have seen in chapter three, Descartes did perform experiments, or at least referred to experiences in his correspondence with Mersenne to justify what has been called his "contest model of impact" (*e.g.* flattening lead bullets with hammers on cushions). However, there is no evidence that Descartes's rules themselves were directly derived from or supported by "Trials of Balls meeting one another" as Sprat claims. Nevertheless, Sprat described them as if they were. Moreover, according to Sprat, Descartes's conclusions were questionable because of the low quality of his experiments (merely using common tennis and billiard balls). In this passage Descartes serves as Wren's foil. For both the "doctrine of motion" is important for establishing the first principles of philosophy, and both attempted to do so through experiments. But, contrary to Descartes, Wren produced and used an *instrument,* which had the capacity to represent the effects of

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[520] Ibid., 312.

a large variety of collisions. Wren's instrument reportedly produced "unexpected effects." And Wren was able to bring all the effects from the various collisions and unexpected results under a theory, which he in turn "confirm'd by many hundreds of *Experiments*" using his instrument. In other words, according to Sprat's account, the Doctrine of Motion is (or, ought to be) established through the proper use of experimental instruments. Descartes's conclusions are questionable—not because they were derived and supported in the wrong way (*i.e*. because they were "rationalist" as opposed to "empirical")—but because of the low quality of the experiments. An alternative type of study, such as that which is described in chapter three of this dissertation, was not even considered as a possibility by Sprat.

Sprat's emphasis on Wren's use of instruments, however, is quite right. As is well documented, Wren had a keen interest in designing and building models, mechanical devices, and instruments. There is, for example, a large catalogue of Wren's "Inventions, Experiments, and Mechanick Improvements" which he exhibited "at the first Assemblies at Wadham-College in Oxford for the Advancement of Natural and Experimental Knowledge."[521] He made models of planets[522] and artificial eyes.[523] He designed new engines for various activities such as raising water and piercing rock in mining. He

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[521] Wren, *Parentalia*, 198-9.
[522] Ibid., 210-11. His solid model of the moon attracted particular attention, and the King himself commanded Wren to perfect it and present it to him at Whitehall. This is mentioned in letters from both Sir Robert Moray and Sir Paul Neile sent in 1661. The model is described as follows: "He has essay'd to make a true *Selenography* by Measure; the World having nothing yet but Pictures, rather than Surveys or Maps of the *Moon.* [...] He has composed a *Lunar Globe*, representing not only the Spots and various Degrees of Whiteness upon the Surface, but the Hills, Eminences and Cavities, moulded in solid Work. The Globe thus fasioned into a true Model of the *Moon,* as you turn it to the Light represents all the menstrual Phases, with the Variety of Appearances that happen from the Shadow of the Mountains and Vallies."
[523] Ibid., 209. "He contrived an *artificial Eye*, truly and dioptrically made (as large as a Tennis-Ball) representing the Picture as Nature makes it: The *Cornea*, and *Crystalline* were Glass, the other *Humours*, *Water*. He took an exact Survey of an Horse's Eye, measuing what the *Spheres* of the *Crystalline* and *Cornea* were, and what the Proportions of the Distances of the *Centers* of every *Sphere* were upon the *Axis*: the Projection in triple the Magnitude, was presented to Sir *Paul Neile*, and the Experiment occasionally reiterated."

created instruments for surveying, drafting, recording the weather, and making music. Several devices in the catalogue are summarized under the heading, "New facile exact Ways of Observation." This heading captures two of the important properties for which Wren's devices were praised by his contemporaries. They made difficult things easier by reducing the dependence on skill,[524] and his instruments were praised as being exact and even infallible.[525]

Wren's interest in instruments can be explained in part by the tradition of mathematics to which he was exposed, namely the English tradition of "practical mathematics." This had been developing primarily outside of the universities by practitioners such as Robert Recorde, John Dee, and Thomas Diggs, through the second half of the 16th century in response to a demand for improved techniques of navigation, fortification, surveying, and cartography.[526] It can be summarized succinctly in two tenets: mathematics is *certain* and mathematics is *useful*.[527] Almost no emphasis was placed on demonstrations, theoretical mathematics, or metaphysical foundations.

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[524] Two famous examples of Wren's devices that made difficult things easier are the following: Wren's "Instrument for drawing any Object in Perspective," which reduced the need for skilled practice of drawing free-hand. This is described in detail in Christopher Wren, "The Description of an Instrument Invented Divers Years Ago by Dr. Christopher Wren, for Drawing the Out-Lines of Any Object in Perspective," *Philosophical Transactions*, 4 (1669): 893-9. The other of Wren's famous devices is his "weather-clock," which recorded, by itself, temperature, barametric pressure, rainfall, and wind direction over time. Skilled observation of meteorological phenomena was not just reduced, but the observer was eliminated altogether. See Wren, *Parentalia*, 198, 207-10.

[525] Wren, *Parentalia*, 209, 198. For example, Wren is described as having converted "a thirty-six Foot Glass...*Tube*" into an *"Astronomical Instrument"* which was used to produce *exact* pictures and surveys of Saturn and the Moon. A description that captures both the alleged "ease" and "exactitude" (and even infallibility) of Wren's instruments is the following: "[Wren] has invented many Ways to make *astronomical Observations* more accurate and easy: he has fitted and hung *Quadrants, Sectants,* and *Radii*, more commodiously than formerly: He has made two *Telescopes*, to open with a Joint like a *Sector*, by which Observers may infallibly take a Distance to half Minutes, and find no Difference in the same Observation reiterated several Times; nor can any warping or luxation of the Instrument hinder the Truth of it."

[526] E. G. R. Taylor, *The Mathematical Practitioners of Tudor & Stuart England* (New York: Cambridge University Press, 1954), 9-10, 26-48.

[527] J. A. Bennett, *The mathematical science of Christopher Wren* (New York: Cambridge University Press, 1982), 7.

Understandably, this rather *instrumental* mathematical tradition arose in tandem with the development of instruments, such as those used in astronomy, navigation, and surveying. Although Wren's interest in models and mechanical devices may date to his childhood, one explicit source of Wren's familiarity with the English tradition of "practical mathematics" is John Wilkins, who was the Warden of Wadham College Oxford while Wren was studying there,[528] and who had a significant impact on Wren both socially and intellectually, by including him in the natural philosophical meetings at Wadham, and by working with him directly.[529] Wilkins's *Mathematicall magick* (1648), published the year before Wren entered Wadham, argued to scholars (rather than practitioners) that "practical mathematics" was worthy of intellectual study.[530] It was written in two books. The first, called *Archimedes*, was on mechanics. The second, called *Daedelus*, was on contrivances, inventions, and "divers kinds of autonoma."

Wren's authority on the topic of collision, for members of the Royal Society, stems from his early experiments and theory, which date to 1661. Although Wren's early experiments with his instrument are referred to effusively in Sprat's *History of the Royal Society*,[531] alluded to by Birch and the register of the Society,[532] and recalled in the pages

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[528] Wren entered Wadham in 1649, earned his BA in 1651 and his MA in 1654.
[529] Bennett, *Mathematical science of Wren*, 12, 18.
[530] John Wilkins, *Mathematicall Magick: or, the wonders that may be performed by mechanical geometry, in two books, concerning mechanical powers and motions, being one of the most easy, pleasant, useful (and yet most neglected Part) of the Mathematics, not before treated of in this language*, in *The Mathematical and Philosophical Works of the Right Rev. John Wilkins, late Lord Bishop of Chester. To which is prefixed the author's life, and an account of his works*, vol. 2 (London: C. Whittingham, 1802), 89-246.
[531] Sprat, *History*, 311-2.
[532] Birch, *History* 2: 335. December 17, 1668. "Dr. Wren produced his theory of the collision of bodies, together with some papers containing the various trials made long before to verify that theory. It was read, and ordered to be registered, the author affirming, that he had this hypothesis several years before, when the society began to be formed; and that Mr. Rooke and himself made divers experiments before the society to verify the same: which affirmation of his was seconded and confirmed by several of the members, who were eye-witnesses of those experiments, as the president, Sir Paul Neile, Mr. Balle, and Mr. Hill."

of Oldenburg's correspondence[533] as well as Huygens's diary[534] and correspondence,[535] the actual records of Wren's experiments and early theory do not seem to have been preserved. However, there is considerable evidence that Wren had developed a theory[536] and had used an instrument or a contrivance of colliding pendulum bobs to investigate collision. Our knowledge of Wren's work comes from secondhand reports and recollections of the 1661 meeting with Huygens. Some of these accounts are recorded many years later. Nevertheless, there is agreement about certain factors such as the use of pendulum bobs, the material of the bobs, that the pendulums were raised to a specific number of degrees, that the size of the bobs were assigned weights measured in numerical values rather than described according to their relative size, and that the apparatus was designed not just to investigate collision but to also confirm or disconfirm predictions. And, in the case of Huygens's involvement, algebra was used to make these predictions. Huygens's use of algebra is significant. Wren too was well versed in algebra and would use symbolic equations in a novel way in his paper on the theory of collision for the Royal Society, *Lex naturae de collisione corporum*. This is a point to which we will return below.

2.2 – Wren and Huygens: Experimental verifications and mathematical demonstrations

Once Wren's *theory* was published in 1669, it was readily recognized as correct and in agreement with Huygens's *theory*—just as Huygens's was thought to be correct

[533] OCH 2: 561 (Moray's account elicited from Oldenburg in October 1665). OCH 2: 624 (another account from Moray, 27 November 1665). HOC 5: 547 (Oldenburg to Spinoza, 18 December 1665). OCH 5: 126-7 (Huygens to Oldenburg, 3 November, 1668).
[534] HOC 22: 573.
[535] HOC 6: 383, 386 (Huygens to Gallois, 18 March 1669). HOC 16: 204 (Huygens to Gallois, 18 March 1669, appendice to previous letter).
[536] Using various letters and Rooke's death (Wren's friend colleague), Bennett claims that Wren's theory of collision dates to 1661.

and in agreement with Wren's. Writing to Oldenburg from Paris before the publication of the papers on collision, Huygens himself acknowledged that Wren's theory was true and consistent with his own:

> The laws of motion [written by Wren] which you did me the favor of sending in exchange for my own are, as you will have doubtless observed, in entire agreement with mine, and these are certainly the correct ones.[537]

However, Huygens immediately went on to question the manner in which Wren justified his laws:

> I very much hope to learn whether Mr. Wren has also looked for some demonstration of them and to see what method he has made use of for that end, or else whether he has only established the law of nature which he proposes in this subject on the basis of experiment.[538]

Wren's lack of demonstrations was seen as a weakness by not just Huygens, but William Neile and Gottfried Leibniz as well.[539]

Wren and Huygens held quite different views on the role of experiment in establishing truths. Huygens seems to have been content to have his theories tested by experiments and he made successful predictions of the behavior of the colliding pendulum bob contrivance in London. However, his rules were to be justified not by experiment, but by demonstrations from previous theorems and axioms, as we have seen in the previous chapter. This is even indicated in the language Huygens used to describe the roles of demonstrations and experiments. For example, in a letter included with his *Regles du mouuement dans la rencontre des corps* for the *Journal des sçavans*, Huygens wrote that his "theory *accords* perfectly with experiments," but it is "*founded* on good

[537] OCH 5: 360. Huygens to Oldenburg, 27 January 1668/9. Translation by Hall OCH 5: 362.
[538] Ibid.
[539] OCH 5: 263, 344, 347, 360-2, 363. OCH 6: 270. OCH 7: 65-6, 162-6.

demonstrations."[540] Wren, on the other hand, seems to rely on the collective experiments with his instrument, and he does not appear to have provided any demonstrations of his theory as Huygens had.[541] Proponents of Wren's work, such as his colleague Sprat, consistently wrote that Wren's theories were "established" and "confirm'd" by the experiments with the instrument. Some historians, such as Bennett, have suggested that Wren's theory is nothing more than "a mere synthesis of experimental results, constrained in its expression by certain unspoken regulative principles of simplicity and symmetry" and emphasize the tradition of "practical mathematics" which was thought to be *certain* and *useful* even without demonstrations or foundations.[542] As we will see in a later section, Wren's theory is more than a synthesis of experimental results, and the mathematics and principles contained therein are more than "regulative principles" to organize and constrain the experimental results. But he did not employ demonstrations in the manner of Huygens, he used symbolic algebra, and experiments with instruments seem to have been an important way to establish truths for Wren.

Wren himself responded to Huygens's question regarding whether demonstrations or experiments are the basis of his laws. Oldenburg forwarded Wren's response in the post script of the letter Oldenburg sent to Huygens confirming the receipt of Huygens's

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[540] HOC 16: 179. *Mais je vous diray seulement que ma Theorie s'accorde parfaitement avec l'experience, & que je la crois fondée en bonne demonstrations, comme j'espere de faire voir bien-tost en la donnant au public*.

[541] Huygens papers published with the *Journal des sçavans* and the *Philosophical Transactions* were summaries of his theory and did not include demonstrations of any of his claims. Huygens did, however, provide the Royal Society with some demonstrations of his propositions. See HOC 6: 336-43. Huygens to Oldenburg, 5 January 1668/9. Huygens's unpublished *De motu corporum ex percussione* (written in 1656, but posthumously published in 1703) provides extensive demonstrations of his propositions. See HOC 16: 30-91.

[542] Bennett, *Mathematical science of Wren*, 72, 118. Hall was notably in opposition to such a view. He wrote: "[f]or what it is worth, it seems to me highly unlikely that this formulation could have been derived from experiment alone, thought it may have been facilitated by some experiments [...]. At least the title of the paper in *Phil. Trans.* speaking of "Dr. Wren's thoery . . . verified by many Experiments . . . " is unambiguous." See Hall, "Mechanics and the Royal Society," 32.

theorems on motion. The letter also confirmed that many in the Royal Society thought that Huygens's theory was correct and in agreement with Wren's.[543]

> Mr. Wren says that, in his opinion, there is no demonstration of what he has proposed in his writings on motion, unless one assumes a large number of other postulates which would perhaps require other demonstrations. However several of our Fellows, and especially our President, Lord Brouncker, very much approve of your method of throwing light on your theorems.[544]

Wren does not directly answer Huygens's question, but rather responds to Huygens's query with a challenge of his own. He notes that any demonstration would require principles that themselves would require demonstration, and presumably this pursuit of more fundamental principles and demonstrations may continue without end.

On the same day that Oldenburg sent the letter containing Wren's reply to Huygens, "two experiments were made with balls to verify Dr. Wren's laws of motion" at the 4 February meeting of the Royal Society:[545]

> 1. Two equal balls, whereof one was let fall from the degree of 12, the other from that of 6, after the impulse moved with contrary velocities, vis. that of 12 with 6, and that of 6 with 12 f*erè*.
> 2. Two unequal bells [sic], which were in weight to one another as eight to one, after the impulse moved with a proportionate velocity. Falling both from the same hight 12 and 12, the bigger returned to 2 1/2, and the smaller 11 1/2.
> Falling both from 4 1/2 the bigger returned to 3 1/2, the smaller to 12 1/2.

Bennett explains Wren's disagreement with Huygens on the importance of demonstrations by emphasizing the influence on Wren of the English tradition of "practical mathematics." In this tradition, mathematics is certain, and is perhaps the only means to attain certain knowledge. This is a view that Wren himself explicitly held.[546]

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543 OCH 5: 371. Oldenburg to Huygens, 4 February 1668/9. "[I]t was first decided that they are equally effective and several among us [the Society] are pretty well persuaded of their truth." Translation by Hall, OCH 5: 373.
544 OCH 5: 373. Oldenburg to Huygens, 4 February 1668/9. Translation by Hall, OCH 5: 375.
545 Birch, *History* 2: 344-5.
546 Wren, *Parentalia*, 200-1. When Wren was elected to the chair of Astronomy at Gresham-College in 1657, his inauguration speech contained the following claim: "Mathematical Demonstrations being built

The certainty and usefulness of mathematics did not require metaphysical or theoretical principles.[547] The role of instruments and experiments, and the tradition of practical mathematics in England are important factors in Wren's work on collision. However, there is much more of importance in the mathematics of Wren's theory than merely a useful tool working in conjunction with an experimental instrument. As we will see, his theory is rooted in principles of economy and symmetry. His brief paper also marks a transition to a symbolic algebraic (rather than geometric) understanding of nature and motion.

Section 3
Mathematics of Collision: The Laws of Motion and the Law of Nature

Christopher Wren, Christiaan Huygens, John Wallis, and all those present at the meetings in London, incorporated the use of instruments into the development and verification of their rules of collision, at least more so than predecessors such as René Descartes and Isaac Beeckman. As we have seen, there were disagreements on the manner by which theories were best supported—whether through demonstration or experiment, as exemplified by Wren and Huygens. We should be careful not to oversimplify the activity of the Royal Society on the topic of collision as being driven purely by experiments, or for the sole purpose of better understanding mechanics. Some

upon the impregnable Foundations of Geometry and Arithmetick, are the only Truths, that can sink into the Mind of Man, void of all Uncertainty; and all other Discourses participate more or less of Truth, according as their Subjects are more or less capable of Mathematical Demonstration."

[547] Bennett, *Mathematical science of Wren*, 119. "[T]ypical of the mathematical sciences in England—'Vitruvian' in character rather than 'Platonic' ... [m]athematics was more a tool to be applied than a privileged source of special enlightenment."

members were deeply interested in the very nature of body and motion,[548] and several others, including both Huygens and Wren, expressed that an understanding of collision could provide insight into the fundamental constituents of nature.[549] The papers presented to and published by the Royal Society were not simply lists of experimental data. None of the papers makes any mention of the experimental set-up or the results of experiments with the instruments. The papers do, however, each have their own internal mathematical justifications.

The balance provides the quantitative relations in both Wallis's and Wren's mathematical justifications of their theories of collision. Wallis focuses on the forces that are required to move bodies on the model of the lever. Wren emphasizes the equilibrium and symmetry of the balance. Wallis's theory is not governed by conservation principles, and although it describes a contest between *impetus* and *impedimentum* (similar to the Cartesian contest between moving force and force of resistance), the contest does not provide the conditions of rebound as it does for Descartes. "Springyness" (and the proposed force of restitution, which explains this spring) is the source of rebound. Unlike Wren and Huygens, Wallis categorizes different kinds of bodies. Absolute hardness and

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[548] Jalobeanu, "Cartesians of the Royal Society," 103-29. Also see the above quoted correspondence between Neile and Wallis.

[549] In the preface Huygens planned for *De motu corporum ex percussione* (1656) he wrote: "For if the whole of nature consists of certain particles, from the motion of which all the diversity of things arises, and by the extremely rapid impulse of which light is propagated and spreads through the immense spaces of the heavens in a moment of time, as many philosophers deem probably, this examination [of nature] will seem to be helped no small amount it the true laws by which motion is transferred from body to body be made known." HOC 16: 150. Translation by Westfall, *Force in Newton's Physics*, 147. In 1657 Wren gave his inauguration speech at Gresham College upon becoming Professor of Astronomy. In the speech he drew a connection between the microscope and the motion of the parts of nature: "For natural Philosophy having of late been order'd into a geometrical Way of reasoning from ocular Experiment, that it might prove a real Science of Nature, not an Hypothesis of what Nature might be, the Perfection of Telescopes, and Microscopes, by which our Sense is so infinitely advanc'd, seems to be the only Way to penetrate into the most hidden Parts of Nature, and to make the most of the Creation." Wren, *Parentalia*, 204-5. For more on the relationship between microscopes and mechanics for Wren see Bennett, *Mathematical science of Wren*, 71-6. And, as has been quoted above, it is notable that in 1667 Sprat explicitly linked Wren's work on "the doctrine of motion" to the "first Principles of Philosophy." Sprat, *History*, 312.

absolute elasticity are mutually exclusive. Also, unlike Wren and Huygens, Wallis engages directly with the problem of transdiction between colliding pendulum bobs and the collision of minute bodies.

Both Wallis and Wren express their mathematical theory of collision with algebra. In keeping with the previous chapters of this dissertation, I focus attention on the contemporary mathematics and show that the technicalities of the mathematics shaped fundamental concepts. Wallis and Wren's theories are of particular interest, since they are among the first *published* algebraic expressions of fundamental physical relationships. This is in contrast to Huygens's classical mathematical demonstrations in the tradition of Archimedes, and in contrast to Descartes's virtual lack of mathematical expressions and demonstrations of any kind in his account of collision. Wallis presents the directionality of motion and impetus with + and – signs. In so doing he provided a legitimation of an otherwise "impossible" quantity by an appeal to physical applications, and presented a new quantitative expression of contrary motion. Wren not only uses the algebraic rules of addition and subtraction to express contrary motion, but also places an epistemological value on the economy of the symbol, and embraces the brevity and unity of expression made possible by algebraic mathematics. Common among both Huygens's and Wren's theories of collision, although in different forms, is the importance of symmetry. As we will see, the theories of collision, developed and discussed in the Royal Society at the end of the 1660s, mark a transition in "the mathematization of nature."

3.1 – John Wallis: the *specious arithmetic* of the forces of collision

On 22 October 1668 members of the Royal Society proposed that the laws of motion should be determined.[550] The following week Oldenburg requested Wren and Huygens to share their work on collision. At the 12 November meeting, spurred by this renewed interest, more "experiments of the communication of motion [were] tried by a contrivance." Hooke was interested in the hypothesis that "no motion dies, nor is any motion produced anew," which he was ordered to think upon to devise new experiments that could test the hypothesis. At the same meeting Moray suggested a relationship between hardness, "springiness," and rebound. And Collins was asked to review "all authors who had written on that subject ["the nature, principles, and laws of motion"]... particularly DesCartes, Borelli, and Marcus Marci: and Mr. Oldenburg was desired to write to Dr. Wallis, that he would take a share of this work."[551]

Two weeks later "Mr. Oldenburg produced a paper of Dr. Wallis, written by him Nov. 15, 1668, at Oxford, concerning the general laws of motion; which was ordered to be registered." This paper was Wallis's "Summary of the Laws of Motion,"[552] which would be published along side Wren's *Lex naturae de collisione corporum* in the *Philosophical Transactions.* The paper was an abbreviated version of a more extensive account of motion in *Mechanica sive de motu tractatus geometricus* (1670), which would be published in the following year.[553] At this same meeting, Hooke reported on experiments conducted on the collision of both "springy and not springy bodies," and suggested that "the reflection of motion depends upon the springiness of bodies; so that

[550] Birch, *History* 2: 315.
[551] Birch, *History* 2: 320.
[552] *Philosophical Transactions* 3 (1668): 864-5. OCH 5: 164-7, 167-70.
[553] John Wallis, *Mechanica, sive, de motu, tractatus geometricus, pars prima* (Londini: Pitt, 1670).

where there is no spring, there can be no reflection." This is a point on which Wallis agreed, although he would dispute Hooke's priority. Dr. Croune, on the other hand, "suggested, that it might be considered, whether the business of motion might not be made out without taking in the notion of the springiness of bodies"[554]—this seems to have been the strategy taken by both Huygens and Wren. The following week, 5 December, William Neile anonymously posed a set of Queries regarding the rules of collision, which would spark a lively correspondence between him and Wallis—covering topics such as conservation, the nature of hardness and elasticity, the problem of transdiction,[555] and the force of resistance in a resting body. These three texts—"Summary of the Laws of Motion," *Mechanica*, and Wallis's correspondence (primarily with Neile via Oldenburg)—serve as my primary sources on Wallis's theory of collision.

Wallis's work on collision is similar to Huygens's and Wren's only in so far as it was published together with theirs. Like Wren's, it used symbolic algebra, which is significant. However, it does not rest on the same mathematical ideals of symmetry and brevity. Rather, it places emphasis on the forces that cause motion, and the "springy" (or *non*-springy) nature of bodies involved in collision. Unlike Huygens and Wren, Wallis displays in his texts a willingness to engage in natural philosophical questions regarding motions and bodies, similar to those considered by Descartes. Perhaps as a consequence of these reflections, Wallis held a contrary attitude regarding "conservation." He fully supported the notion that motion can "perish." Although Huygens acknowledged that his own theory and Wren's coincide, when asked about Wallis's theory of collision, he wrote:

[554] Birch, *History* 2: 328.
[555] Maurice Mandelbaum, *Philosophy, Science and Sense Perception: Historical and Critical Studies* (Baltimore: Johns Hopkins Press, 1964), 88-112. Of course, neither Wallis nor Neile use the word "transdiction."

"I do not know if Mr. Wallis will have been able to reduce his rules to the same meaning as ours, because I do not see much of a connection."[556]

Wallis had very little formal mathematical education.[557] He did, however, have a proclivity for languages and cryptanalysis. He acquired proficiency in Latin, Greek, and Hebrew at a young age,[558] but his primary interest was in Divinity. In addition to earning his degrees at Cambridge, he was ordained in 1640 at age 24.[559] While a private chaplain to Lady Vere,[560] he was casually shown, "between jeast and earnest," a cipher, which had been acquired after the Parliamentarian victory at the siege of Chichester in December 1642—the first months of the English Civil War.[561] Wallis deciphered the message "in about 2 hours time."[562] After this unexpected success he decoded many Royalist ciphers for the Parliamentarians.[563]

In the midst of the disruptions of the war, Wallis lived in London. Beginning in 1645, he met with Wilkins, Goddard, Glisson, Merret, Foster, and Hank at Gresham College and sometimes at Goddard's lodging to discuss "The New Philosophy or Experimental Philosophy."[564] In 1647 Wallis read Oughtred's *Clavis mathematicae*—the

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[556] OCH 5: 556. Huygens to Oldenburg, 19 May 1669.
[557] Christoph J. Scriba, "The Autobiography of John Wallis, F. R. S.," *Notes and Records* 25 (1970): 29-30. Also see Scott, *Mathematical Work of Wallis*, 3-4.
[558] Scott, *Mathematical Work of Wallis*, 3.
[559] Ibid., 6.
[560] Lady Vere was the widow of Lord Horatio Vere (1565-1635).
[561] Scriba, "Autobiography of Wallis," 37.
[562] Ibid., 37.
[563] Ibid., 38.
[564] Ibid., 39-40. "Our business was (precluding matters of Theology and State Affairs) to disco{urs} and consider of *Philosophical Enquiries*, and such as related thereunto; as *Physi{ck}, Anatomy, Geometry, Astronomy, Navigation, Staticks, Magneticks, Chymicks, Mechanicks,* and *Natural Experiments;* with the State of these Studies, as then cultivated, at home and abroad. We there discoursed of the *Circulation of the Bl{ood}, the Valves in the Veins, <the Venae Lacteae, the Lymphatick vessels,>* (28) *the Copernican Hypothesis, the Nature of Comets, and New Stars, the Satellites of Jupiter, the Oval Shape* (as it then appeared) *of Saturn, {the} spots in the Sun, and its Turning on its own Axis, the Inequalities and Selenograp{hy} of the Moon, the several Phases of Venus and Mercury, the Improvement of Telescopes, and grinding of Glasses for that purposes, the Weight of Air, the Possibility or* ||p. 48|| *Impossibility of Vacuities, and Natures Abhorrence thereof; the Torricellian Experiment in Quicksilver, the Descent of*

same text with which Wren became familiar through his mentors, and the analytical contents of which Wren had applied to Galileo's work and then his own work on collision, as we will see in section 3.2. After Wallis's encounter with the *Clavis mathematicae*, came an outpouring of mathematical activity. Around 1648/49 Wallis, Wilkins, and Goddard moved to Oxford and continued to meet, while those left in London also continued their meetings regarding the *Experimental Philosophy*.[565] In 1649 Wallis was appointed Savilian Professor of Geometry at Oxford, a post he would keep for over 50 years.[566]

The mathematical work of John Wallis was distinctive, and plays an important role in his theory of collision. Among his many contributions, and the many features of his mathematics, Wallis argued for an arithmetic understanding of algebra (a *specious arithmetic*), which was supposed to be more fundamental than geometry—a universal mathematics. More than many of his contemporaries, he made a concerted effort to legitimize both negative quantities and the roots of negative quantities. This was done, in part, by an appeal to their usefulness in physical applications. His *specious arithmetic*, including its negative quantities, was the system in which Wallis presented his theory of collision and thus his "laws of motion."

In what follows, I first focus on Wallis's account of force, which is rooted in his understanding of simple machines. He presents this using his distinctive interpretation of algebra. Before addressing his theory of collision, which is intended to be founded on this

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heavy Bodies, and the degrees of Acceleration therein; and divers other things of like nature. Some of which were then but New Discoveries, and others not so generally known and imbraced, as now they are; With other things appertaining to what hath been called *The New Philosophy*; which, from the times of *Galileo* at *Florence*, and *S^r. Francis Bacon (Lord Verulam)* in *England,* hath been much cultivated in *Italy, France, Germany,* and other Parts abroad, as well as with us in *England.*"

565 Ibid., 40.

566 Scott, *Mathematical Work of Wallis*, 13-14.

notion of force, I examine Wallis's discussion of the nature of bodies. In doing so, the "hard" bodies described by Wallis are contrasted with competing notions of hardness as well as "springyness" found in both the ideas of his contemporaries and predecessors. Wallis's account of collision is not governed by prior commitments to the conservation of motion, or the Cartesian quantity of motion, or any other conserved quantity, as Descartes's is. Wallis's clear rejection of the conservation of motion is closely connected to the contrary relationship between absolute hardness and the "force of elasticity." The third section discusses Wallis's theory of the collision of absolutely hard bodies, which is described in terms of a contest between two forces: *impetus* and *impedimentum.* This returns the discussion to the topics from the third chapter of the dissertation, such as the contest model of force, the force of resistance in a body at rest, and the distinction between motion and rest. Wallis's "contest" is significantly different from Descartes's. Wallis was not committed the conservation of the moving force, nor was his explanation of rebound rooted to the failure of the moving force to overcome the force of resistance as it was for Descartes. Rather, motion stops when it fails to overcome the force of resistance, and "springyness," *i.e.* the force of restitution, was the source of rebound for Wallis. The correspondence between Neile and Wallis indicates that the problem of "transdiction" was at the heart of the debate on the force of resistance of a body at rest. The fourth section returns to Wallis's "Summary of the Laws of Motion," published with the *Philosophical Transactions*, to examine the intertwined relationship between Wallis's *specious arithmetic* and his theory of collision—particularly the directionality of *impetus* and speed. We will find that Wallis's "Summary" presents a thoroughly algebraic theory of collision.

3.1.1 – The Foundation of all Machines for Facilitating Motion

The "Summary of the Laws of Motion" in the *Philosophical Transactions* encapsulates Wallis's thoughts on collision in 14 enumerated sections. In the first eight, Wallis develops what he called the "foundation of all machines for facilitating motion:"

> For in whatever ratio the weight is increased, the speed is diminished in the same ratio; whence it is that the product of the weight and the speed for any moving force is the same.[567]

"Moving force" is slightly ambiguous here. Wallis is not precise in his meanings of force, and Wallis uses several words sometimes interchangeably for what appear to be his notions of force, including *vis, momentum,* and *impetus*.[568] Nevertheless, in the context of the first 8 sections of his paper for the *Philosophical Transactions*, force (*vis*) is identified with the agent that causes a weight (*pondus*) to move. At this point he does not claim that a moving weight has a force equal to the product of its weight and degree of speed (roughly similar to the Cartesian quantity of motion, or the modern notion of momentum), although later in his paper, without explanation, he will.

In the sections subsequent to the first eight, Wallis uses this "foundation of all machines for facilitating motion" to describe collision. The laws of motion are to be found in the collision of bodies; and the principles governing collision are the same as the principles governing all machines. Collision is thus presented as and presumed to operate as a kind of machine. Wren, who also had a keen interest in machines and other contrivances, notably makes no mention of forces. Wren's account of collision is founded on the underlying principles of the balance, focusing on symmetry and equilibrium.

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[567] Wallis, "Summary Account of the General Laws of Motion," 864-5. Translation by Hall, OCH 5: 168.
[568] Scott, *Mathematical Work of Wallis*, 104, 108-109; Also see Westfall, *Force in Newton's Physics*, 236-9.

Wallis's account, on the other hand, relies on the moving powers in simple machines such as the lever.[569]

"The foundation of all machines for facilitating motion" is expressed in the following proportion, which Wallis derived in sections 2, 3, 4, 5, and 6. First note the following: *V* (*vis*), force; *P* (*pondus*), weight; *C* (*celeritas*), speed; *m,* "any rational exponent"[570]

$$V.PC :: V.mP \times \frac{1}{m}C = PC$$

This proportion can be read as follows: *V* is to *PC* as *V* is to the product of mP and $\frac{1}{m}C$. And that product is equivalent to *PC*.

The germ of this idea is in Wallis's second section, which relates force, weight, distance, and time:

> 2. Therefore, if a force *V* moves a weight *P*, a force *mV* will move *mP*, *caeteris paribus*, that is through the same distance in the same time, or with the same speed.[571]

It is likely that Wallis had simple machines in mind, such as the lever, when he wrote this. Consider that a force (sometimes referred to as a power) is required to move a weight sitting at the end of a lever. The force will move the weight some distance in some

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569 Westfall, *Force in Newton's Physics*, 234-6.
570 Wallis, "Summary Account of the General Laws of Motion," 865. Translation by Hall, OCH 5: 168. I have cited the algebraic symbols as they appear in the original Latin transcriptions as found in the *Phil. Trans*., and the OCH, rather than Hall's edited "translations" of the algebraic symbols found in Hall's English translations in the OCH. I follow this convention each time I cite the algebra found in the OCH. For instance, I reproduce "." to indicate a ratio, rather than the more familiar ":" symbol. Wallis appears to be using the word "exponent" to indicate the "exponent of the ratio." According to *Phillips's New World of Words*, 6th edition (1706), the term is defined as follows: "*Exponent of the Ratio* or Proportion between two Numbers or Quantities, is the Quotient arising, when the Antecedent is divided by the Consequent. Thus 6 is the Exponent of the Ratio that 30 has to 5." I have not found the term in prior editions of either Edward Phillip's *New World of English Words* (1st ed., 1658) or Thomas Blount's *Glossographia* (1st ed., 1656).
571 Wallis, "Summary Account of the General Laws of Motion," 864. Translation by Hall, OCH 5: 168, with slight changes drawn from the original by me. After Hall's translation of this statement, he provided a footnote to exclaim: "A perfectly Aristotelian principle!" See OCH 5: 170n.

time, depending on the constraints of the machine. A proportionally larger force will be required in order to move a larger weight. If the machine itself is not modified, *i.e.* if the fulcrum of the lever is not moved, the larger force will move the larger weight through the same distance in the same time as the previous weight and force. This appears to be the root of Wallis's notion of force: it is the power required to move a weight by a machine, such as a weight at the end of a lever.

Presumably 2 rests on his first statement, which asserts that any effect is proportional to its cause. This also clarifies Wallis's machine notion of force, weight, and distance/time.

> 1. If the agent is as *A*, and the effect as *E*; with the agent as 2*A* the effect is as 2*E,* with 3*A* as 3*E*, all things being equal; and universally with *mA* as *mE*, *m* being any rational exponent.[572]

Thus the force is the agent and the effect is the motion of the weight. In his larger work *Mechanica*, Wallis makes this explicit: I call a power that produces motion [*potentiam efficiendi motum*] motive force or simply force..." just as he calls "a power that is contrary to motion or resists motion Resistance or Force that resists [*vim resistendi*]."[573] Prior to these definitions, Wallis also similarly defined *momentum* and *impedimentum*: "I call that which aids in producing motion *Momentum*, and that which prevents motion or impedes it *Impedimentum*."[574]

Note Wallis's expression of proportionality using a symbolic variable. Although seemingly simple, the transition between proportions and algebraic products was

572 Wallis, "Summary Account of the General Laws of Motion," 864. Translation by Hall, OCH 5: 167-8.
573 Wallis, *Mechanica*, 3. Translation by Westfall, *Force in Newton's Physics*, 231.
574 Wallis, *Mechanica*, 2. Translation by Westfall, *Force in Newton's Physics*, 231.

contentious, and is a distinctive feature of Wallis's mathematics of collision.[575] Wallis, following Aristotle, argued that arithmetic was more universal and abstract than geometry, since a unit, unlike a geometric point, has no position.[576] However, Wallis parted with Aristotle and many others in identifying arithmetic with algebra. This may seem innovative and modern, but Wallis himself, who was among the first to write a history of algebra, was convinced that algebra was ancient.[577] As Savilian Professor of Geometry at Oxford (1649) Wallis gave a series of lectures, which in 1657 would be published as *Mathesis universalis: sive Arithmeticum opus integrum.* According to these lectures, algebra (the *arithmetica speciosa*) had nearly universal application. It could be applied to geometry ("reduc[ing] the procedures of geometrical constructions to algebraic operations"), it could better express Eudoxus's theory of proportions in book V of Euclid's *Elements*, and he thought it could be used to investigate the relationships between *continuous* physical magnitudes.[578]

In section 3 of the "Summary of the Laws of Motion", Wallis states that "if something moves through a distance *L* in time *T*, in time *nT* it will move through a

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575 Chikara Sasaki, "The Acceptance of the Theory of Proportions in the Sixteenth and Seventeenth Centuries," *Historia Scientiarum* 29 (1985): 83-116.

576 Aristotle, *Posterior Analytics*, I-27, 87[a] 33-38. "Knoweldge at the same time of the fact and of the reasoned fact, as contrasted with knowledge of the former without the latter, is more accurate and prior. So again is knowledge of objects which do not inhere in a substrate as contrasted with that of objects which do so inhere (*e.g.*, arithmetic and harmonics) and that which depends upon fewer factors as contrasted with that which uses additional factors (*e.g.*, arithmetic and geometry). What I mean by additional factors is this: a unit is a substance without position, but a point is a substance with position: I regard the latter as containing an additional factor." Translation by Hugh Tredennick, E. S. Forster, *Posterior Analytics, Topica,* Loeb Classical Library (Cambridge: Harbard University Press, 1960), 152-5. Also see Sasaki, "Theory of Proportions," 93-4.

577 Wallis, *Treatise of Algebra*, 3. Wallis begins Chapter 2, which is entitled, "Of Algebra in Euclid, Pappus, Diophantus, and in the Arabic Writers," with the following claim: "It is to me a thing unquestionable, That the Ancients had somewhat of like nature with our *Algebra*; from whence many of their prolix and intricate Demonstrations were derived. And I find other modern Writers of the same opinion with me therein." He goes on to provide an extensive argument. Also see Jacqueline A. Stedall, *A Discourse Concerning Algebra: English Algebra to 1685*, (New York: Oxford University Press, 2002).

578 Sasaki, "Theory of Proportions," 93-4.

distance *nL*."[579] Later in 5 he defines "degrees of speed" *C* as "proportional to the distances passed over in the same time... $\frac{L}{T}.C :: \frac{m}{n}\frac{L}{T}.\frac{m}{n}C$ That is, the degree of [speed] is as the distances directly and the times reciprocally."[580]

Using these relations for (constant) *motion* in 3 and *force* from 2, Wallis sets up a proportion between force, time, weight, and distance (*longitudinem*):

> 4. And so, if the force *V*, in time *T*, moves the weight *P* though the distance *L*, the force *mV* in the time *nT* will move *mP* thought a distance *nL*.[581]

Relying on his particular algebraic interpretation of proportions, the proportion in 4 is then converted into a product:

> And moreover as *VT* (the product of the force and the time) is to *PL* (the product of the weight and the length) so is *mnVT* to *mnPL*.[582]

Thus, we have the expression $VT.PL :: mnVT.mnPL$. In section 6 Wallis presents a purely algebraic manipulation to show that force is equal to the product of weight and degree of speed:[583]

> 6. Accordingly, because $VT.PL :: mnVT.mnPL$, $V.\frac{PL}{T} :: mV.\frac{mnPL}{nT}$; that is,
> $V.PC :: mV.mPC$
> $= mP \times C$
> $= P \times mC.$

Section 7 provides a gloss on this expression. (1) If a force *F* can move a weight *P* with some speed *C*, then a force *mV* will move the same weight with a proportional velocity *mC*,[584] (2) or it will move a proportionally larger/smaller weight *mP* with the same

[579] Wallis, "Summary Account of the General Laws of Motion," 864. Translation by Hall, OCH 5: 168.
[580] Ibid.
[581] Ibid.
[582] Ibid.
[583] Ibid.
[584] $V.PC :: mV.P \times mC$

velocity *C*.[585] (3) Or it will move "any weight with such a velocity that the product of weight and velocity is *mPC*."[586]

And to these two variations (and general statement) of force and the product of weight and degree of speed, can be added the expression, which Wallis takes to be the foundation of all machines:

$$\begin{aligned} V.PC &:: mV.mPC \\ &= V.mP\times\tfrac{1}{m}C \end{aligned}$$

> In whatever ratio the weight is increased, the speed is diminished in the same ratio; whence it is that the product of the weight and the speed for any moving force is the same.

Force is a fundamental concept in Wallis's account of collision. This distinguishes Wallis's theory from the other two famous papers by Wren and Huygens on the topic published nearly simultaneously in the *Philosophical Transactions.* For Wallis force (*vis*) is the agent that causes a weight (*pondus*) to move through a distance in some time. This agent is akin to the power that causes a machine, such as a lever, to lift a weight. Using his distinctive interpretation of algebra, Wallis derives his "foundation of all machines" with this notion of force at its core. It is on this foundation that Wallis builds his account of collision.

3.1.2 – The Nature of Bodies and "Whether no Motion in the World perish"

Beeckman, Descartes, Huygens, and Wren all focused their attention on specific kinds of bodies in their theories of collision. Most called their bodies "hard," however what constituted a "hard" body for each investigator differed significantly. Beeckman's

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[585] $V.PC :: mV.mP\times C$
[586] $V.PC :: mV.mPC$

bodies were perfectly hard atoms, but they could not rebound. Descartes's hard bodies, described in his rules of collision, did rebound in some instances; absolute "hardness" was defined by all the parts (parts which are indefinitely divisible) being at rest with respect to each other. Unlike Beeckman, some instances of the collision of these hard bodies involved rebound. This was explained not by a force of elasticity, but rather by the conservation of quantity of motion and the contest model of force. If the force of resistance is greater than the force of motion, the body in motion retains its motion but changes its direction. Huygens's bodies were also hard, but they bore almost no resemblance to either those of Descartes or Beeckman. Huygens treated his hard bodies as if they were (what we would call) perfectly elastic. However, he provided no explanation for how this could be, nor did he investigate the nature of bodies in this context. Wren too left the topic untouched. He did not even specify whether his bodies were "hard." However, like Huygens, Wren treated his bodies as if they were (what we would call) perfectly elastic. And in subsequent experiments prosecuted at the Royal Society, it would be recorded that Wren's rules were best confirmed by experiments with "the most springy bodies."[587] With the exception of brief statements made in passing by Descartes and Beeckman, all of these individuals built their theories around one kind of body, or more precisely, around one understanding of one kind of body, which differed from person to person.

Wallis, on the other hand, classified different kinds of bodies, and developed his ideas on the collision of different kinds of bodies. Similar to Beeckman, Wallis thought

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[587] Birch, *History* 2: 347. 18 February 1668/9. "The experiments of motion were prosecuted with springy bodies, by which it appeared to some of the members, that the laws of motion, established by Dr. Wren, were best verified by the motion of the most springy bodies. These experiments were ordered to be continued at the next meeting."

that a perfectly hard body must be perfectly *inelastic*. If a body is not perfectly hard, Wallis explained that it would be either elastic or soft. As he mentioned briefly in the "Summary of the Laws of Motion:"

> If the bodies that thus collide are taken to be not absolutely hard...but as yielding to the shock although able to restore themselves by an elastic force, it will come about that such bodies may rebound from each other which otherwise would move along together (and indeed rebound more or less, as this restoring force is greater or less)....[588]

"Springy" bodies change shape upon impact, but return to their shape and separate after impact due to an elastic force.[589] Soft bodies, on the other hand, also change shape. In these, however, some motion is absorbed upon deformation and there is no force to cause them to separate.[590] The mathematical theory developed in Wallis's "Summary of the Laws of Motion" in the *Philosophical Transactions* involves perfectly hard bodies. Wallis discusses the collision of bodies, wherein an elastic force restores their shape and causes rebound, in the *Mechanica*, chapters *De percussione* and *De elatere et resilitione seu reflexione*.[591]

In correspondence with Oldenburg, Wallis maintained that he had precedence over others (*e.g.* Hooke) on the matter of elasticity or "springyness" as the cause of rebound.[592] As we have seen, Hooke was in the midst of prosecuting several experiments

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[588] Wallis, "Summary Account of the General Laws of Motion," 866. Translation by Hall, OCH 5: 170.
[589] Wallis, *Mechanica*, 686. "I call that by which a body deformed by force strives to restore its original figure Elastic Force." Translation by Westfall, *Force in Newton's Physics*, 242.
[590] Wallis, *Mechanica*, 661-2. Westfall, *Force in Newton's Physics*, 240.
[591] In what follows I emphasize Wallis's work on hard bodies in the "Summary of the Laws of Collision" rather than his propositions on "springy bodies" in *Mechanica*. For an overview of Wallis's proposition in his chapter *De elatere et resilitione seu reflexione*, see Scott, *Mathematical Work of Wallis*, 121-3.
[592] OCH 5: 265n. Hooke had stated his view at the 29 October 1668 meeting: "if there were to be had a body absolutely hard, and destitute of all springiness, if would not rebound at all..." Also see Birch, *History* 2: 216. "Mr Hooke moved, that experiments might be made to see, whether all hard bodies, that rebound, do not so upon the account of having springy particles in them; and that it might be inquired into, whether there be any body springy upon any other score, than that it has air in it. He conceiving, that if there were to be had a body absolutely hard, and destitute of all springiness, it would not rebound at all, and it being

to investigate the role of "springyness" in collision. On 5 December 1668 Wallis wrote to Oldenburg:

> What you say was stated in the Society (but not, by whom,) *That the springyness of Bodies is the* Onely *cause of their rebounding*. My opinion is, (& hath been a good while, & oft declared,) that (beside *Repercussion* which I suppose was not intended to be excluded, being one manifest cause; as when a Racket returns the Ball;) there is no other cause (that I know of) of Rebounding, but Springyness. And therefore, you see, in my Hypothesis sent you, I assign no other.[593]

Perhaps Wallis's slightly irritated tone is connected to the fact that he had clearly reminded Oldenburg of this very position only two days before.[594] Moreover, three days after Sir Robert Moray suggested at the 12 November meeting of the Royal Society that a connection might be found through experiment between hardness and rebound[595]—a meeting in which others suggested that bodies with no springiness should be experimented upon to see "how much that quality [springiness] contributed to the rebounding"[596]—Wallis sent his "Summary of the Laws of Motion" to Oldenburg in which the connection between elastic force and rebound is clearly stated, as well as absolute hardness and absolute inelasticity.

Again, on 21 December, Wallis even more forcefully defended his claim to priority on this notion against Hooke, and explained how he himself was "forced" to

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said, that such a body would not be easily found for making the experiment, he answered, that it might be tried comparatively."

593 OCH 5: 220. Wallis to Oldenburg, 5 December 1668.

594 OCH 5: 218. Wallis to Oldenburg, 3 December 1668. "That all rebounding comes from Springynesse, is my opinion; & therefore you see mee express that as the onely reason in my short Hypothesis which you lately had."

595 Birch, *History* 2: 320. "Sir Robert Moray moved, that bodies might be provided of several degrees of hardness, and of the same matter and weight, as steel bodies, and the like, to see whether the harder they are, the more they will rebound".

596 Birch, *History* 2: 320.

accept the notion "*that springyness is the cause of rebounding*" as a consequence of the principles of his mechanics.[597]

> I was forced to it by the necessity of a consequence from those principles which I layd down in my first & second chapter (allready printed) where is couched the foundations of those demonstrations (or calculations rather) which are to bee deduced in some following chapters, De Percussione, et Motuum Acceleratione, &c. in their due places. The reason which forced mee to bee of that opinion, you have breefly, in the Hypothesis I sent: For if the force *prorsum* (supposing bodies perfectly hard) do (as is here argued) require that both it, & the body it directly strikes, should moove the same way (that way which the greater force determines) and both at the same swiftness; I found, that, upon this account, there could be no rebounding: and therefore was necessitated (as you see in my hypothesis) to have recourse, to that of Elasticity, in one or both of the bodies. And, if the rest of the hypothesis bee admitted; this, seemes to mee unavoidable. [...] [I]t is a notion which, I think, I have well digested; & am very confident it must hold.[598]

As will be discussed more completely below, Wallis, using his "foundation of all machines," shows that after a hard body strikes another, both move together with the same speed. Although Wallis's mathematics is more sophisticated, this account of collision of hard bodies is essentially the same as Beeckman's and Descartes's early view.

Contrary to Hooke and Wallis, and perhaps in agreement with Moray, William Neile supposed that "the harder [a body] is the more spring it has and for my part I [Neile] think a diamond (or what ever body is the hardest in nature) has a stronger spring then [sic] other bodies and a greater quantity of motion in it."[599] The latter clause regarding "quantity of motion" refers to Neile's position that "a body cant be made hard without motion in its particles" and that "the more [internal] motion it has the more

[597] OCH 5: 274-5. Wallis to Oldenburg, 21 December 1668. "The other suggestion of Mr H[ooke]; *that springyness is the cause of* rebounding; is (as I sayd before) not new to mee; & I think you are my witness that you had it in my hypothesis of motion, before he started it in the Society; I am sure, before you signified any such thing to mee: so that I suppose he doth not think mee to have robbed him of his motion. How hee came by it (whether by a conjecture or a certainty) hee knows best."
[598] OCH 5: 274-5.
[599] OCH 5: 264. Neile to Oldenburg, 18 December 1668.

spring it has."[600] However, for Neile the important task is to determine the fundamental motions of particles that compose hard bodies. He thought this was a task best suited for "reason" rather than "experiment," since one cannot perceive "minute particles" and one cannot be sure that they behave in the same manner as the bodies that can be perceived."[601]

Neile suggested that hardness, and thus the cause of rebound, could be explained by the motion of the minute particles composing a body. Hooke and Wallis, who thought that absolutely hard bodies were not springy, explained rebound by positing an additional force—elasticity—that also accounted for the restitution of bodies that had been deformed by impact. The motion of minute particles could not be observed, and Wallis acknowledged that he did not have an explanation for how an elastic force worked (just as he accepted that gravity existed, but had no explanation of it).[602] Neither Wallis nor Hooke seems to have come to accept the relationship between hardness and elasticity empirically. Hooke realized the difficulty, or perhaps impossibility, of finding an

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[600] Ibid.

[601] OCH 5: 518, 542. Neile to Oldenburg, 7 May 1669 and 13 May 1669. "if the motion of small particles be too obscure and too uselesse a thing I am not much concerned perchance all philosophye is so too it is the more likely to be so if there can be not certaintye found in the principles of it. I desire to know the nature of motion and the nature of quiet I desire not only to know that if here be two bodies of considerable magnitude moving against one another they shall reflect with such a swiftnesse for that they may doe and yet motion may not reflect from motion when it moves with it and to know a thing barely by experiment is good for use but it is not science or philosophye." And the following week Neile wrote: "...you know my desire is if I could only find some firmenesse in the foundations the Superstructures I confesse it will passe my Skill to carry much further than the very beginnings... *...the foundations themselves I think are to be grounded upon reason for I doubt experiment will hardly ever cleare the nature of motion in minute particles...* ...for very probably there is no quantity of matter liable to sense but does farre exceed the magnitude of those divisions and subdivisions which are made by motion in minute particles... ...sense may tell us that a whole considerable quantity of matter is moved out of its place but sense will not tell us after what fashion the motion is performed in the minute particles of that matter or whether it were with intervalls of rest or no" (italics added).

[602] OCH 5: 287-8. Wallis to Oldenburg, 31 December 1668. "What that is which wee call springynesse; & what, Gravity: I do not determine: but from those things, what ever they are, & from what ever causes they do proceed, I am to give account of the effects, I ascribe to them. The one (from what-ever cause) is the principle of the motion of restitution; & the other, of tendency downward. I know Des-Cartes & others do attempt to assign causes of both; but I have not yet seen any hypothesis that doth fully satisfy my apprehensions; & therefore I do not, as to that, determine anything."

example of an absolutely hard body to experimentally test the relationship between hardness and springyness, and suggested that it might be done "comparatively."[603] And as mentioned, Wallis reported that he was "forced" to the positions that springyness was the cause of rebound and that an absolutely hard body would not rebound, by the principles of his mechanics.

Wallis's particular position on the nature of the bodies involved in his mathematical rules of collision is closely connected to his position on the lack of conservation of motion. The topic of the conservation of motion was explicitly discussed in letters between Neile and Wallis. On December 5th Wallis provided a preliminary response to four queries that had been sent to him anonymously by way of Oldenburg.

> 1. *Whether Quiescent Matter have* [sic] *any resistance to motion ...*
> 2. *Whether Motion may pass out of one Subject into another ...*
> 3. *Whether no Motion in the World perish, nor new motion be generated ...*
> 4. *Whether different motions meeting, destroy one another. ...*[604]

The queries originated from William Neile, who had sent them before encountering either Wallis's or Wren's theories of collision. Wren produced his theory of collision (as well as several old papers "containing the various trials made long before to verify that theory") at the 17 December meeting of the Royal Society. At the same meeting additional experiments were tried on collision and springyness.[605] In a letter Neile sent to Oldenburg

[603] Birch, *History* 2: 216.
[604] OCH 5: 220-1. Wallis to Oldenbrug, 5 December 1668.
[605] Birch *History* 2: 335. "An experiment was made in prosecution of the motion, that springiness is the cause of rebounding; vis. a wooden globe was let fall against wood, a gut-string, and a brass-wire. In the first case the rebounding was languid, and of a very short duration; in the second, it was much stronger, and more durable; in the last, strongest and most durable of all. Which was conceived to proceed from the different degrees of force of the spring in the several bodies employed. Mr. Hooke took occasion to mention, that he thought, that air, next to quicksilver, gave the quickest and most forcible reflexion; and that the sparkling of diamonds in rings proceed from the air left behind the stones. Dr. Wren produced his theory of the collision of bodies, together with some papers containing the various trials made long before to verify that theory. It was read, and ordered to be registered, the author affirming, that he had this hypothesis several years before, when the society began to be formed; and that Mr. Rooke and himself made divers experiments before the society to verify the same: which affirmation of his was seconded and

the day after the meeting, Neile mentioned that he had not yet seen Wallis's paper on collision, and criticized Wren's theory.[606] In the same letter, Neile also revealed his own positions regarding the queries.

Three days later, Wallis replied through Oldenburg to clarify and elaborate his response, specifically his response to the third question, which regarded conservation. Upon reading Neile's own position on the topic, Wallis restated the question as follows:

> *Whether no Motion in the World perish, &c;* that is, (as you now explain it,) *whether any of that motion, that was first* (or at any time since) *impressed in matter be lost, or* (onely) *communicated from one parcell of the matter to another; so that though this or that body do cease to be moved, that the motion itself ceaseth or perisheth not.*[607]

Wallis's response was, in short, that "motion may be extinguished." He acknowledged that others have been uncomfortable with this answer:

> [It] is a question, which I find Mathematicians, as well as Naturalists, sparing to determine positively; and, you know, I am sparing and wary in asserting Universall Negatives. Yet you have, to this, my answer, full inough, (if it be observed,) in my answere to the fourth. For I there intimate my judgement, that *motion may be extinguished*, & I shew you *how*; that is, a Motion compounded of two contrary forces, may be extinguished by each other, & become equivalent with Rest.[608]

The cause of rebound is elasticity. Hard bodies are not elastic. When two equal hard bodies moving with the same speed in contrary directions collide, motion is extinguished. In other words, it is the case that the motion, which was first impressed in matter, can be

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confirmed by several of the members, who were eye-witnesses of those experiments, as the president, Sir Paul Neile, Mr. Balle, and Mr. Hill. Mr. Hooke was ordered to take care, that the experiments be made before the society, to verify the several cases relating to the theory produced. He was desired to bring in what he had considered of the cause of springiness."

[606] OCH 5: 263-4. W. Neile to Oldenburg, 18 December 1668. "I wish Dr. Wren would explain his principles a litle [sic] more fully but he is against finding a reason for the experiments of motion (for ought I see) and says that the appearances carrie reason enough in themselves as being the law of nature. I think it is the Law of nature that they should apear but not without some causes. [...] I am sorry I doe not at present know Dr Wallises hypothesis of motion."

[607] OCH 5: 274. Wallis to Oldenburg, 21 December 1668.

[608] Ibid.

lost. It is not the case that it is only communicated from one parcel of matter to another. Wallis is quite clear. Motion in the World perishes.

3.1.3 – The Contest of impetus and impedimentum

In sections 1 through 8 Wallis developed his "foundation of all machines," that is, that "the product of the weight and the speed for any moving force is the same," which means that "in whatever ratio the weight is increased, the speed is diminished in the same ratio." In sections 9, 10, and 11 he uses this foundation to account for three different kinds of collisions of hard bodies. In 9 a weight collides directly with a motionless weight. In 10 a weight collides directly with another weight that is traveling in the same direction as the first, but with a different speed. In 11 two weights collide directly from opposite directions. Wallis's theory involves a contest between two forces: *impetus* and *impedimentum*, which is significantly different from the contest of Marci and Descartes, namely Wallis is not committed to a prior principle of conservation, and his source of rebound is in "springyness" rather than the force of resistance. In Wallis's correspondence with Neile, we find renewed debates on the force of resistance in a body at rest. These reveal basic disagreements on methodology, appearance and reality, and the very purpose of a study of collision among members of the Royal Society.

10 and 11 are built from the case of a weight P colliding with a motionless weight mP, as described in section 9. In this chief case of collision, since hard bodies do not rebound, after they meet, the weight is increased to $P + mP$ or equivalently $(1 + m)P$. Because of the foundation of all machines: "as the same force is constrained to move a greater weight, the velocity will be diminished in the same proportion. Thus, $V.PC ::$

$V.\frac{1+m}{1}P\times\frac{1}{1+m}C = PC$"[609] This means that after collision they will both move with a velocity of $\frac{1}{1+m}C$. Both the writings found in Isaac Beeckman's *Journal*, and Descartes's early work on collision had come to the same conclusion. However, Beeckman and Descartes represented this notion using only specific numerical examples. Wallis, on the other hand, expresses it abstractly and generally using algebraic symbols.

The concept of force in the "foundation of all machines" was built on the notion of an agent that produces the effect of putting a weight in motion. As we have already seen, in the context of the first eight sections culminating in this "foundation," it has been called the "moving force"—the force that causes a weight to move. In section 9 Wallis identifies "the product of the weight and the velocity" (*Factum ex Pondere et Celeritate*) with the "*impetus*" of the bodies after collision. This quantity is similar to the modern notion of momentum or the Galilean notion of *impeto* (or *momento*).[610] However, Wallis provides no justification or explanation of the shift from force as "agent that puts a weight in motion" to force as the "power of a weight in motion" in his paper for the *Philosophical Transactions*. Wallis has extended discussions of *force*, *momentum*, and *impetus* in his *Mechanica*. However, historians such as Westfall and Scott have noted that these discussions are neither the paragon of clarity nor consistency.

In addition to the "force of motion" which in general terms Wallis calls *momentum* (which is the force responsible for putting a weight into motion), and the *impetus* (which is the power of a weight in motion), Wallis, in his *Mechanica*, also discusses a force of resistance which in general terms he calls the *impedimentum*—"that which prevents motion or impedes it." Wallis begins chapter XI, entitled *De Percussione*,

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[609] OCH 5: 165. Translation by Hall, OCH 5: 168.
[610] See chapter 3.

of his *Mechanica* (1670-1) with the following proposition, which stipulates the relationship between the *impetus* (*i.e.* the force "equal in power to the body so moved") and the *impedimentum* (*i.e.* "force of the obstacle resisting the motion"):

> PROP. I. If a heavy body in motion is considered as perfectly hard, and if it directly strikes a firm hindrance or obstacle that is also perfectly hard; and if a force equal in power to the body so moved is less than the force of the obstacle resisting the motion, or even equal to it, the motion will be stopped.
>
> But if it is greater, the obstacle will be surmounted and the motion continued, but retarded or diminished in that ratio which the resistance of the obstacle demands, which a calculation will establish.
>
> That is, if the force required to overcome the obstacle is subtracted from the *momentum* (which is composed of the weight and the speed) and the remainder, whatever it may be, is understood to be divided by the weight, the degree of speed remaining will be found.[611]

A contest between the *impetus* and the *impedimentum* determines the outcome of a body striking another. Wallis's analysis of the collision of hard bodies found later in his chapter, *De Percussione,* from his *Mechanica*, and the summary presented in sections 9-11 of Wallis's paper for the *Philosophical Transactions,* are consistent with this general proposition. Recall that Marcus Marci's account of collision relied on a contest between impulse and resistance, just as Descartes's relied on a contest between moving force and the force of resistance. However, Wallis's contest is significantly different from either Marci or Descartes. For both Marci and Descartes, when the force of resistance is not overcome, the moving body rebounds.[612] Descartes held as a prior principle that the quantity of motion is conserved. When the "moving force" does not overcome the force of resistance (of a body at rest, for example), the body in motion rebounds retaining all of its motion. The quantity of motion before and after impact is the same. Wallis does not hold as a prior principle that the "quantity of motion" (or *impetus*) is conserved. For

[611] Wallis, *Mechanica*, 660. Translation by Westfall, *Force in Newton's Physics*, 239-40. Also see Wallis, *Mechanica*, 18 (chapter I, proposition XI). Translation by Scott, *Mathematical Work of Wallis*, 111.
[612] See chapter 5 section 5.2.1.

Wallis, when the moving force does not overcome the force of resistance (if the force of resistance is greater than the moving force or equal to it) the moving body stops. Descartes's conservation principle together with his contest view provided him with the conditions of rebound ("without this, no body would ever be reflected by encountering another"[613]). Wallis, on the other hand, explained rebound in terms of "springyness" and the force of restitution.

3.1.3.1 – Appearance and Reality: weights, minute bodies, and the force of resistance

Among the queries Neile had posed to Wallis was whether or not a body *at rest* had a force of resistance.[614] Notoriously, in Descartes's later view of collision, the "force of resistance" in a resting body could not be overcome by the force of any smaller moving body.[615] Using different understandings of both (relative) motion as well as body from Descartes's, Huygens argued against the Cartesian rules of collision.[616] Contrary to Huygens, both Wallis and Neile agreed that there was a meaningful difference between rest and motion.[617] The question of the force of resistance in a body at rest was still a live issue for them.

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[613] AT IV 184. Descartes to Clerselier, 17 February 1645. Translation by Garber, *Descartes' Metaphysical Physics*, 261.
[614] Birch, *History* 2: 333. Also see OCH 5: 218. Neile had posed the question, which was the first of his four queries, in the following way: "*Whether Quiescent Matter have any resistance to motion.*"
[615] This topic, the contest between the force of motion and the force of resistance, has been examined previously in this dissertation, particularly in chapter 3. Also see Gabbey, "Force and inertia."
[616] See above, particularly chapter 4, and the section on Huygens in this chapter.
[617] As noted in a previous section of this chapter, Wallis seems to have incorrectly conflated Descartes's and Huygens's ideas on motion. In a letter (OCH 6: 189. Wallis to Oldenburg, 15 August 1669), Wallis wrote that he did not think motion was only relative: "'Tis very little I have to say in answere to yours of Aug. 4 [...] nor to the note inclosed concerning M. Huygens, with whom I do for the most part concur, though not in all. For I am not yet satisfyed in that notion of Descartes, which hee seems to imbrace, that Motion is onely relative; & , of the two bodies separated, it is indifferent whether of the two be sayd to move." In the correspondence between Wallis and Neile, they extensively discuss both rest and motion. An example of a

Explicitly following Hobbes,[618] Neile claimed in his "Hypothesis of Motion," which was partially presented to the Royal Society at the 29 April 1669 meeting,[619] and sent to Wallis via Oldenburg on 8 May 1669,[620] that *quiescent matter* offers *no resistance* to motion. In his paper he was quick to clarify that "it is very obvious that all bodies known to us possess th[e] faculty of resisting more or less," including those bodies that appear to us to be at rest. According to Neile, bodies offer resistance because of the "internal motion of the most minute particles in every body." But truly *quiescent matter*, *i.e.* a minute particle at rest, offers no resistance.

Wallis and Neile's correspondence was extensive. It would be a mistake to assume that the correspondents were talking past each other, even though Wallis was concerned with the forces involved in the impact of "hard weights," whereas Neile was concerned with fundamental minute particles, which could be said to be at rest (with no internal motion). Wallis maintained his same position against Neile *after* reading Neile's clarifications in his "Hypothesis of Motion." Wallis's position was essentially the same

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particularly fraught discussion on the distinction between rest and motion can be found in the postscript of letter 1064, (OCH 5: 313-4. Neile to Oldenburg, 2 January 1668/9).

[618] William Neile, "William Neile's Hypothesis of Motion," OCH 5: 521. "This part of the theory I gladly acknowledge taking from the books of Mr. Hobbes. Since it may be gathered from what has gone before that wherever there is resistance, or a body's reaction, there motion is to be found, for that is the unique cause able to produce such an effect naturally because of the incapacity of matter at rest to impede motion in any way, since matter does assuredly yield to any body that strikes it, without any reluctance or repugnancy, and since it is very obvious that all bodies known to us possess this faculty of resisting more or less, it is on that account quite fitting that we attribute motion to them. And since this resistance is opposed in all directions to any external impetus whatever, that internal motion of the most minute particles in every body must exist in an almost endless variety, by which they resist any external impulse.." Translation by Hall, OCH 5: 525.

[619] Birch, *Hypothesis* 2: 361, 2. "Mr. William Neile was desired to produce his theory of motion; which being done, it was read, and ordered to be registered. After some discourse upon this theory, the author was desired to complete it, and to consider how to verify his principles by experiments, and to accommodate them to the rules of Dr. Wren and Monsr. Huygens; which he promised he would endeavour to do." At the 6 May 1669 meeting, Neile was called upon again to complete his "theory concerning the principles of motion, and [apply] them to the rules given by Dr. Wren and Monsr. Huygens for experiments." Neile replied that he had not yet finished.

[620] OCH 5: 519-524. "William Neile's Hypothesis of Motion" was written in Latin. English translation by Hall, OCH 5: 524-8.

analysis found in section 9 of his Summary for the *Philosophical Transactions*,[621] wherein a hard body at rest does offer resistance. Recall that after a weight P moving at speed C collides with a body mP at rest, they both move together with the reduced speed $\frac{1}{1+m}C$. Moreover, although Wallis's "hard weights" may not be identical to the "minute particles" of Neile, they were also not the same as the mundane weights from experience. Neither Wallis's nor Neile's objects of collision were observable.

Throughout his correspondence with Wallis via Oldenburg, Neile is concerned with what has since come to be known as the problem of transdiction. According to Neile both the nature of bodies (whether they are hard, soft, or elastic), as well as the ability of a body to offer resistance to another, are *apparent* in phenomenal bodies, but those qualities are explained by the *real* motion of minute particles. The minute particles may not share the same properties of hardness/softness/elasticity or resistance, as the bodies observed in experiments, since those properties are explained by the motion of minute particles, and since the minute particles are not observable. This poses a methodological difficulty: how to determine the laws of motion of the fundamental components of reality, if one only has experimental access to the behavior of apparent bodies. Neile suggests that their only recourse is to investigate the unobservable minute particles by "reason"

[621] OCH 5: 220. Wallis to Oldenburg, 5 December 1668. Initially, before Wallis read Neile's "Hypothesis of Motion" and had only read the then anonymous queries, Wallis responded to the question in a way that implies that he thought it was a question regarding inertia: "I look upon it as taken for granted by most of our moderns; that...it is indifferent as to rest or motion... And accordingly doth remain as it is, either in rest or motion, & this with the same direction & celerity, till some positive cause alter it." Later, after Wallis had examined Neile's "Hypothesis of Motion," he first extensively clarified his position, before referring in general terms to the ideas found in section 9 of his "Summary." See OCH 5: 541. Wallis to Oldenburg, 10 May 1669.

rather than by "experiment." He does concede, however, that any theory of motion (based on reason) should be consistent with the sets of experiments performed by the society.[622]

For example, in defense of the approach he took in his "Hypothesis of Motion" Neile claims that if one does not take seriously the "motion of small particles" and does not first attempt to clarify by reason the questions "if it be not true that motion stoppes motion" and "if it be not true that quiescent matter has no resistance to motion," then "there can be no certaintye found in the principles of it."[623] Neile claimed to be interested in the "firmenesse in the foundations," "the Superstructures." And methodologically: "the foundations themselves I think are to be grounded upon reason for I doubt experiment will hardly ever cleare the nature of motion in minute particles."[624] According to Neile, the study of phenomenal bodies, such as wooden pendulum bobs colliding, in order to understand nothing more than the motion of wooden pendulum bobs, may be "good for use but it is not science or philosophy."[625]

A distinguishing feature of Wallis's work on collision, is his emphasis on forces and the causes of motion. He took into account the different natures of bodies (hard, soft, elastic). Notably he was the first, or among the first, to identify the much discussed concept of elasticity in the Royal Society as the force of restitution in bodies and the

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[622] OCH 5: 265, 312, 363, 542, 559. Neile extensively discusses "appearance and reality." See OCH: 286-7, 347, 364, 518, 542, for his discussions of "reason and experiment."
[623] OCH 5: 517-8. W. Neile to Oldenburg, 7 May 1669.
[624] OCH 5: 542. W. Neile to Oldenburg, 13 May 1669.
[625] OCH 5: 518. W. Neile to Oldenburg, 7 May 1669. "if the motion of small particles be too obscure and too uselesse a thing I am not much concerned perchance all philosophye is so too it is the more likely to be so if there can be no certaintye found in the principles of it. I desire to know the nature of motion and the nature of quiet I desire not only to know that if here be two bodies of a considerable magnitude moving against one another they shall reflect with such a swiftnesse for that they may doe and yet motion may not reflect from motion when it moves with it and to know a thing barely by experiment is good for use but it is not science or philosophye."

cause of rebound in collision.[626] This distinguishes him from Descartes's attempts at discussing rebound, as well as Huygens and Wren. For the latter two, bodies rebound, but Huygens and Wren do not explain why the bodies rebound. Nevertheless, in the eyes of Wallis's correspondent Neile, Wallis's work, if it only treated "bodies of considerable magnitude," was *neither* science *nor* philosophy. Or, if Wallis's work was intended to describe the fundamental motions of reality, then his account failed to reconcile appearance and reality, or at least failed to justify the "Superstructures" by neglecting to defend an account of weight and resistance, which Neile had done by appealing to the motion of minute particles.

Clues to Wallis's intentions regarding this issue of "transdiction" can be found in two instances: 1.) The hard bodies described by Wallis are theoretical rather than observable. The hard bodies used in experiments at the Royal Society rebound, and even Hooke (who shared Wallis's view on hardness and elasticity) admitted the difficulty and perhaps impossibility of finding an absolutely hard body with which to perform the experiments. Thus, the as of yet unobservable, absolutely hard bodies described by Wallis are not the weights perceived in an experiment. Recall too that according to Wallis, his positions on the "force of elasticity" as the cause of rebound, as well as the inelasticity of perfectly hard bodies, was not acquired through experience and experiment. He claimed to have been "forced" to accept them by the principles of his mechanics. 2.) While discussing the relationship between the physical causes of motion

[626] OCH 5: 303. Wallis to Oldenburg, 2 January 1668/9. However, Wallis mentions in a letter to Oldenburg regarding the priority squabble with Hooke, that he has since seen this notion suggested in several books, including Borelli's *De vi percussionis*, page 47, and "as I remember in a French writer which Mr Collins lent me [Wallis] when I was last in London." He had forgotten the name of the book and the French writer, but remembered that "he had three volumes in a broad quarto."

and the mathematical rules of collision, in a set of letters to Oldenburg,[627] Wallis claims that motive force and the force of resistance are essentially fundamental. The moving force and the force of resistance must be postulated if motion is postulated at all:

> As to what you say of the Physical cause of motion: If it be onely, Why this way? & thus fast? & with thus much force? the Mathematical hypothesis satisfyes. But if it be Whence it comes to pass that there [is] any Motion in the World? I doubt wee must make that for a Postulatum; That there is Motion, as well as, That there is Matter. And refer both to the same Original cause. And, if we allow motion to begin, we must postulate that there is a vis motrix ["motive force"] even in resting Bodies. At lest I know not at present what to say more to it.[628]

In short, according to Wallis, *impetus* and *impedimentum* are a direct consequence of the postulation of motion. Resistance (such as weight) is not to be explained, as Neile, Hobbes, and Descartes suggested, by the motion (or rest) of more fundamental particles.

The disagreements on the force of resistance in a body at rest reveals a web of issues, ranging from the very relevance of experiments (such as the collision of wooden pendulum bobs for the determination of the laws of motion), to the status of forces (such as *impetus*, *impedimentum,* and the force of restitution/elasticity), to the relationship between appearance and reality in the fundamental laws of motion. Unlike Neile, who explains springyness, hardness, and resistance by appealing to the motion of minute particles, which subsequently raises methodological challenges for Experimental Philosophy, Wallis posits the necessary existence of forces, such as that of resistance, but provides no explanation of the forces. Unlike Huygens and Wren, Wallis's theory of collision as a contest between forces, and his discussions of this theory, reveal a willingness to explicitly engage in foundational natural philosophical questions.

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627 OCH 5: 221. Wallis to Oldenburg, 5 December 1668. OCH 5: 230-1. Wallis to Oldenburg, 10 December 1668.

628 OCH 5: 230-1. Wallis to Oldenburg, 10 December 1668. I follow Hall's interpretation of *vis motrix* as "motive force." See OCH 5: 231n.

3.1.4 – The Physical legitimation of impossible numbers and the new mathematics of direction: posito + signo Dextorsum, et − Sinistrorsum significante

The cases of collision in which both bodies are in motion, either moving in the same direction (as in section 10 of the "Summary of the Laws of Motion") or in opposite directions (as in section 11) are built from the collision of one body at rest (in section 9). In Westfall's study of Wallis's theory of collision, he mentions that the similar analyses in 10 and 11 are "too obvious to require restatement."[629] Admittedly, the concepts are simple, but section 11 is worth restating and analyzing because it is among the first published texts in which the direction of motion is explicitly indicated using the algebraic negative sign, and the directionality of motion is determined by simplified algebraic expressions which, in final form, do not directly correspond to particular physical quantities such as weight, speed, and *impetus*.

Section 11 is the longest section in Wallis's paper for the *Philosophical Transactions*. I have reproduced Hall's translation in full:

> 11) If weights moving in opposed directions collide or meet each other directly such as the weight *P* (force *V*, velocity *C*) to the right and the weight *mP* (velocity *nC* and so force *mnV*) to the left, the velocity, impetus and direction of both may be worked out in this way.
>
> With the one weight moving to the right, if the other (*mP*) were at rest it would acquire a velocity $\frac{1}{(1+m)}C$ and hence an impetus of $\frac{1}{(1+m)}mPC$, to the right; and the moving body (*P*) would retain this same velocity with an impetus of $\frac{1}{(1+m)}PC$, to the right (from Section 9).
>
> But with the one weight moving to the left, by similar reasoning if the other (*P*) were at rest it would acquire a velocity $\frac{mn}{(1+m)}C$ and hence an impetus of $\frac{mn}{(1+m)}PC$ to the left; and the moving body (*mP*) would retain this same velocity with an impetus of $\frac{mn}{(1+m)}mPC$ to the left.
>
> Now as there is motion in both directions, the impetus of the body which at first moved to the right will be the aggregate of $\frac{1}{(1+m)}PC$ to the right and $\frac{1}{(1+m)}mnPC$ to the left, and so in fact either to the left or the right according to which is the greater, with the impetus that is the difference of these two. That is

[629] Westfall, *Force in Newton's Physics*, 240.

(expressing movement to the right as positive [+] and movement to the left as negative [−][630]), the impetus will be

$$+\frac{1}{(1+m)}PC - \frac{1}{(1+m)}mnPC$$

and the velocity will be $\frac{1-mn}{1+m}C$ (to the right, or the left, as 1 or *mn* is the greater).

And similarly, the impetus of the body which at first moved to the left will be

$$+\frac{1}{(1+m)}mPC - \frac{mn}{(1+m)}mPC = \frac{1-mn}{1+m}mPC$$

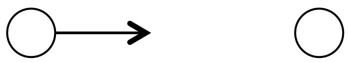
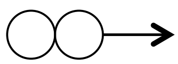
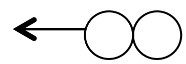
and the velocity will be $\frac{1-mn}{1+m}C$ (to the right, or the left as 1 or *mn* is the greater).[631]

Wallis's strategy for "working out" the velocity, *impetus*, and direction of the weights, relies on successively treating the scenario as if one and then the other of the weights were at rest. In other words he works out the velocity and *impetus* of the collision twice, first treating *mP* as if it were at rest and *P* in motion to the right, and then treating *P* as if it were at rest and *mP* as if it were in motion to the left. This amounts to a simple application of the ideas established in section 9. The moving body has an *impetus.* The body at rest serves as an *impedimentum*. There is an *impetus* and *impedimentum* for both hypothetical cases—if *mP* is taken to be at rest and *P* in motion to the right, and if *P* is taken to be at rest and *mP* in motion to the left.

Wallis's innovation is to identify motion to the right by the + sign, and motion to the left by the − sign. He then takes the aggregate of an *impetus* to the right and an *impetus* to the left. Whichever *impetus* is greater determines the direction after collision. Since an *impetus* is calculated for each weight (*P* and *mP*), and since these *impetus* are worked out for each hypothetical case, there are four *impetus* to choose from in these

[630] OCH 5: 166. In Hall's editorial choices for the English translation of this passage he excluded the algebraic signs that Wallis used in this clause. The original Latin is as follows: "*Hoc est*, (*posito* + *signo* Dextorsum, et − Sinistrorsum *significante,*) *Impetus erit* + $\frac{1}{(1+m)}PC - \frac{1}{(1+m)}mnPC$." Also see *Philosophical Transactions* 3 (1668): 865.
[631] Wallis, "Summary Account of the General Laws of Motion," 865-6. Translation by Hall, OCH 5: 169.

aggregates. The diagram below clarifies the way Wallis worked out velocity, *impetus*, and direction of the collision of weights moving in opposite directions:

Hypothetical Case 1	Hypothetical Case 2
Before collision  *P* *C* *mP (at rest)*	Before collision  *P (at rest)* *nC* *mP*
After collision  $\frac{1}{(1+m)}C$	After collision  $\frac{mn}{(1+m)}C$
"impetus of weight *P*" (initially moving) $+\ \frac{1}{(1+m)}PC$	"impetus of weight *P*" (initially at rest) $-\ \frac{1}{(1+m)}mnPC$
"impetus of weight *mP*" (initially at rest) $+\frac{1}{(1+m)}mPC$	"impetus of weight *mP*" (initially moving) $-\ \frac{mn}{(1+m)}mPC$

Aggregate Impetus

"impetus of the body which at first moved to the right"	
$+\quad \frac{1}{(1+m)}PC$	$-\quad \frac{1}{(1+m)}mnPC$
impetus of weight *P* (initially moving) from Hypothetical Case 1	impetus of weight *P* (initially at rest) from Hypothetical Case 2
"impetus of the body which at first moved to the left"	
$+\quad \frac{1}{(1+m)}mPC$	$-\quad \frac{mn}{(1+m)}mPC$
impetus of weight *mP* (initially at rest) from Hypothetical Case 1	impetus of weight *mP* (initially moving) from Hypothetical Case 2

The link between direction and signs was not obvious. Wallis was among the first to establish the connection. This simultaneously provided an elegant way of describing direction algebraically in a non-visual, non-geometric manner; and it provided an interpretation for an otherwise absurd, "impossible" quantity.

As we have seen above, Wallis was a proponent of the position that the foundations of algebra were arithmetic. He envisioned an algebra "liberated from geometry," and he thought that the abstractness of this "specious arithmetick" allowed for unbounded applications.[632] As Pycior has argued, one of the main purposes of Wallis's *Treatise of Algebra, both historical and practical*, in addition to providing an overview and history of algebra, was to legitimize the hitherto controversial negative and imaginary quantities.[633]

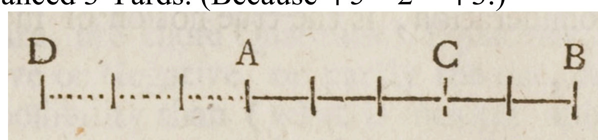
Wallis began his 66th chapter "*of* Negative Squares, *and their* Imaginary Roots *in Algebra*" of the *Treatise of Algebra* with an acknowledgement of the impossibility of negative numbers and the roots of negative numbers. After explaining that it was not possible to multiply any number into itself to produce a negative, he noted that negative numbers were just as impossible:

> but it is also Impossible, that any Quantity (though not Supposed Square) can be *Negative.* Since it is not possible that any *Magnitude* can be *Less than Nothing*, or any *Number fewer than None.*[634]

Although impossible, they are nevertheless not "absurd" and are, in fact, useful. He used a simple example of the direction of a man moving to legitimize negative numbers by appealing to the usefulness of the physical interpretation. Although the concept is now

[632] Pycior, *Symbols*, 118-9.
[633] Ibid., 107.
[634] Wallis, *Treatise of Algebra*, 264.

considered simple enough to be taught in primary school, the Savilian Professor of Geometry at Oxford invested nine paragraphs and a diagram into its explanation:

> Yet is not that Supposition (of Negative Quantities,) either Unuseful or Absurd; when rightly understood. And though, as to the bare Algebraick Notation, it import a Quantity less than nothing: Yet, when it comes to a Physical Application, it denotes as Real a Quantity as if the Sign were +; but to be interpreted in a contrary sense.
>
> As for instance: Supposing a man to have advanced or moved forward, (from A to B,) 5 Yards; and then to retreat (from B to C) 2 Yards: If it be asked how much he had Advanced (upon the whole march) when at C? or how many Yards he is now Forwarder than when he was at A? I find (by Subducting 2 from 5,) that he is Advanced 3 Yards. (Because $+5 -2 = +3$.)
>
> 
>
>
> But if, having Advanced 5 Yards to B, he thence Retreat 8 Yards to D; and it be then asked, How much he is Advanced when at D, or how much Forwarder than when he was at A: I say −3 Yards. (Because $+5 -8 = -3$.) That is to say, he is advanced 3 Yards less than nothing.
>
> Which in propriety of Speech, cannot be, (since there cannot be less than nothing.) And therefore as to the Line AB *Forward*, the case is Impossible.
>
> But if (contrary to Supposition,) the Line from A, be continued *Backward*, we shall find D, 3 Yards *Behind* A. (Which was presumed to be *Before* it.)
>
> And thus to say, he is *Advanced* −3 Yards; is but what we should say (in ordinary form of Speech,) he is *Retreated* 3 Yards; or he wants 3 Yards being so Forward as he was at A.
>
> Which doth not only answer Negatively to the Question asked. That he is not (as was supposed,) Advanced at all: But tells moreover, he is so far from being Advanced, (as was supposed,) that he is Retreated 3 Yards; or that he is at D, more Backward by 3 Yards, than he was at A.
>
> And consequently −3, doth as truly design the Point D; as +3 designed the Point C. Not Forward, as was supposed; but Backward, from A.
>
> So that +3, signifies 3 Yards Forward; and −3, signifies 3 Yards Backward: But still in the same Streight Line. And each designs (at least in the same Infinite Line,) one Single Point: And but one. And thus it is in all Lateral Equations; as having but one Single Root.[635]

Wallis then provided the very same reasoning to interpret the root of a negative number. Instead of contrariness on a line, he provides a physical example of contrariness in a

[635] Ibid., 265.

plain—the acreage of land gained and lost to the sea. Supposing the land lost (a negative plain) to be in the form of a square, this negative square will have a side. And the length of this side can be determined. Thus, although the quantity is an "imaginary root" (which is "impossible") there is a physical interpretation of the root of a negative number.[636]

Wallis did not attempt to legitimize these quantities with mathematical arguments. Rather he appealed to their usefulness in physical applications. But Wallis was also among the first to suggest that contrary directions of motion (or the loss of acreage by the sea) could be understood with the algebraic signs of $+$ and $-$.

The analysis of motion in section 11 of Wallis's "Summary of the Laws of Nature" identifies positive and negative quantities with motion to the right and left, rather than forward and backward. The commonality is the contrary direction of motion. In his "Summary" this is extended to *impetus* as well as motion. The *impetus* of each body is determined by the aggregate of the *impetus* to the right (positive) and left (negative). Notably, Wallis "works out" the direction of motion after collision—*not* by comparing which of the absolute values of the impetus are greater—but rather by solving the

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[636] Ibid., 265-6. "Now what is admitted in Lines, must on the same Reason, be allowed in Plains also.

As for instance: Supposing that in one Place, we Gain from the Sea, 30 Acres, but Lose in another Place, 20 Acres: If it be now asked, How many Acres we have gained upon the whole: The Answer is, 10 Acres, or $+10$. (Because of $30 - 20 = 10$). Or, which is all one 1600 Square Perches. (For the *English* Acre being Equal to a Plain of 40 Perches in length, and 4 in breadth, whole Area is 160; 10 Acres will be 1600 Square Perches.) Which if it lye in a Square Form, the Side of that Square will be 40 Perches in length; or (admitting of a Negative Root,) -40.

But if then in a Third place, we lose 20 Acres more, and the same Question be again asked, How much we have gained in the whole; the Answer must be -10 Acres. (Because $30 - 20 - 20 = -10$.) That is to say The Gain is 10 Acres less than nothing. Which is the same as to say, there is a Loss of 10 Acres: or of 1600 Square Perches.

And hitherto, there is no new Difficulty arising, nor any other Impossibility than what we met with before, (in supposing a Negative Quantity, or somewhat Less than nothing:) Save only that $\sqrt{1600}$ is ambiguous; and may be $+40$, or -40.

We cannot say it is 40, nor that it is -40. (Because either of these Multiplyed into itself, will make $+1600$; not -1600.)

But thus rather, that it is $\sqrt{-1600}$, (the Supposed Root of a Negative Square;) or (which is Equivalent thereunto) $10\sqrt{-16}$, or $20\sqrt{-4}$, or $40\sqrt{-1}$."

equation, determining the speed from the resulting *impetus*, and then comparing two quantities within the simplified expression:

> That is (expressing movement to the right as positive [+] and movement to the left as negative [−][637]), the impetus will be $+\ \frac{1}{(1+m)}PC - \frac{1}{(1+m)}mnPC$ [$=\frac{1-mn}{(1+m)}PC$] and the velocity will be $\frac{1-mn}{1+m}C$ (to the right, or the left, as 1 or *mn* is the greater).[638]

The direction is determined by whether *mn* is greater or less than 1. This is significant. Algebra was not merely a heuristic tool for Wallis, as it was for Huygens. The latter used it in his manuscripts while considering and criticizing the Cartesian rules of collision, but he nowhere used it in his writings on collision, which were prepared for publication. According to Wallis, algebra was not just a heuristic. In Wallis's "Summary of the Laws of Motion" direction is worked out by examining a portion of a simplified algebraic expression, which has been abstracted away from an apparent physical quantity such as motion or *impetus*. For Wallis, the generality of symbolic algebra, *i.e.* "specious arithmetick," endows it with nearly universal applicability.

John Wallis attempted to found his theory of collision on a notion of force drawn from simple machines—the agent that puts a weight in motion. Despite this, in a letter to Oldenburg from 5 December, Wallis responds to a potential criticism of his work. The issue that seems to have been raised is that mathematical rules of collision are merely descriptive; and what is needed is an account of the physical causes of motion:

> ...you tell mee that *the Society in their present disquisitions have rather an Eye to the Physical causes of Motion, & the Principles thereof, than the Mathematical*

---

[637] OCH 5: 166. In Hall's editorial choices for the English translation of this passage he excluded the algebraic signs that Wallis used in this clause. The original Latin is as follows: "*Hoc est*, (*posito* + *signo* Dextorsum, et − Sinistrorsum *significante,*) *Impetus erit* $+\ \frac{1}{(1+m)}PC - \frac{1}{(1+m)}mnPC$." Also see *Philosophical Transactions* 3 (1668): 865.

[638] Wallis, "Summary Account of the General Laws of Motion," 866. Wallis goes on to do the same for the impetus to the left. "And similarly, the impetus of the body which at first moved to the left will be $+\ \frac{1}{(1+m)}mPC - \frac{mn}{(1+m)}mPC = \frac{1-mn}{1+m}mPC$ and the velocity will be $\frac{1-mn}{1+m}C$ (to the right, or the left as 1 or *mn* is the greater)." Translation by Hall, OCH 5: 169.

> *Rules of it*. That the Hypothesis I sent, is indeed of the *Physical* Laws of Motion, but *Mathematically* demonstrated. For I do not take the Physical & Mathematical Hypothesis to contradict one another at all. But what is Physically performed, is Mathematically measured. And there is no other way to determine the Physical Laws of Motion exactly, but by applying the Mathematical measures & proportions to them.[639]

According to Wallis himself, his hypothesis is not merely a mathematical description. It provides the physical laws of motion. His is an account of the forces of collision, where force is the agent that causes motion. Not only is his mathematics and physics consistent, there is, according to Wallis, no way to determine the causes of motion exactly, other than mathematics. The "mathematical measures and proportions" that Wallis "applied" was his distinctive specious arithmetic. He provided a fully algebraic study of the forces of collision.

3.2 – Wren's *Lex naturae:* The Mathematics of Proper and Improper Motion

Wren's *Lex naturae de collisione corporum* is concise in the extreme. It includes a definition, a law, an incredibly economical classification of 13 kinds of collision represented by a handful of simple diagrams, and a set of compact algebraic equations to calculate the final velocities of the bodies upon collision. The main text is contained in five sentences:

> The proper and most truly natural velocities of bodies are reciprocally proportional to the bodies.
>
> The Law of Nature: Hence bodies R, S having their proper velocities retain them even after collision. And bodies R, S having improper velocities are by collision returned to equilibrium; that is, that quantity by which R exceeded and S fell short of their proper velocities before collision is by the collision subtracted from R and added to S, and vice versa.
>
> For this reason the collision of bodies having their proper velocities is equivalent to a balance swinging about its center of gravity.

[639] OCH 5: 221. Wallis to Oldenburg, 5 December 1668.

> And the collision of bodies which have improper velocities is equivalent to a balance reciprocating upon two centers equidistant either side of the center of gravity: for the balance may be extended into a yoke when the need arises.[640]

Despite its debt to extensive experimentation, there is no mention of experiments or the devices used to develop and verify the theory in the document. Although Wren's theory would receive experimental verifications at the Society's meetings, Wren's account itself does not present an experimental justification. It is notable, however, that when Wren read his theory at the 17 December 1668 meeting of the Royal Society, he also produced "some papers containing the various trials made long before to verify that theory."[641] The mechanical device that is described in the document is the balance, which was *not* used in Wren's experiments on collision, rather than the pendulum, which *was used*. And, despite the on going experiments on the role of "springiness" and hardness in collision,[642] he

[640] Christopher Wren, "Lex naturae de collisione corporum," *Philosophical Transactions* 3 (1669): 867-8. It has also been collected in OCH 5: 319-20, with an English translation by Hall, OCH 5: 320-1.

[641] Birch, *History* 2: 335. "Dr. Wren produced his theory of the collision together with some papers containing the various trials made long before to verify that theory. It was read, and ordered to be registered [Register, vol., iv. p. 29], the author affirming, that he had this hypothesis several years before, when the society began to be formed."

[642] Birch, *History* 2: 320. 12 November 1668:

> "Sir Robert Moray moved, that bodies might be provided of several degrees of hardness, and of the same matter and weight, as steel bodies, and the like, to see whether the harder they are, the more they will rebound. Others moved, that bodies might be provided, that had no springiness, or but little, to see, how much that quality contributed to the rebounding."

Birch, *History* 2: 328. 26 November 1668:

> "The experiment devised and made this day by Mr. Hooke was the impelling of wooden balls against both springy and not springy bodies, whereby he intended to evince, that the reflection of motion depends upon the springiness of bodies; so that where there is no spring, there can be no reflection. But the experiment made not being satisfactory to the society for the purpose declared, Mr. Hooke proposed another to be made at the next meeting, viz. with a metalline string made more or less true, to see what the returns of reflections of it will be, according to its several degrees of tension. Dr. Croune suggested, that it might be considered, whether the business of motion might not be made out without taking in the notion of the springiness of bodies."

Birch, *History* 2: 335. 17 December 1668 meeting:

> "An experiment was made in prosecution of the motion, that springiness is the cause of rebounding; vis. a wooden globe was let fall against wood, a gut-string, and a brass-wire. In the first case the rebounding was languid, and of a very short duration; in the second, it was much stronger, and more durable; in the last, strongest and most durable of all. Which was conceived to proceed from the different degrees of force of the spring in the several bodies employed. Mr. Hooke took occasion to mention, that he thought, that air, next to quicksilver, gave the quickest

made no mention of these concepts, nor did he refer to a force of motion or a force of resistance.[643] He does not address whether his notion of motion is relative or absolute, nor does he directly refer to a conservation of any quantity related to motion whatsoever. Wren's theory is presented in spare style. Rather than the confusion of experimental results and the prolix descriptions of experimental apparatuses and methods, Wren's theory exhibits the mathematical ideals of elegance and the economy of both symbolic equations as well as the symmetries of the balance.

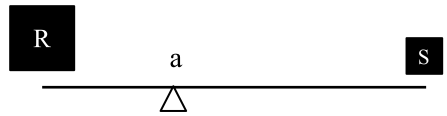
3.2.1 – The Balance of Nature

What Wren calls the "proper" or the "most truly natural" velocities of bodies is actually a relation between the bodies and their respective velocities involved in impact. It is a relation that is literally in balance. The velocities of the bodies are reciprocally proportional to the size of the bodies themselves, in the same way that, when in equilibrium, the lengths of the arms of a balance supporting two bodies are reciprocally proportional to the size of the bodies.

> and most forcible reflexion; and that the sparkling of diamonds in rings proceed from the air left behind the stones."

It was during the 17 December 1668 meeting that Wren "produced his theory of the collision of bodies, together with some papers containing the various trials made long before to verify that theory." Experiments on collision were continued, now to verify Wren's theory (*e.g.* Birch, *History* 2: 344-5. 4 February 1668/9), and by 18 February 1668/9 the "experiments of motion were prosecuted with springy bodies, by which it appear to some of the members, that the laws of motion, established by Dr. Wren, were best verified by the motion of the most springy bodies." See Birch, *History* 2: 347.

643 Westfall, *Force in Newton's Physics*, 203-6. Westfall interprets Wren's *Lex naturae* to be a "dynamic" theory of "elastic bodies," and contrasts this with what he calls Huygens's "kinematic theory" of "hard bodies." Not only is the dynamic/kinematic distinction anachronistic when describing Wren and Huygens's theories, Wren never mentions the notion of "force" nor the notion of "elasticity" in his theory. After attributing the notion of "force" to Wren, Westfall ends his section on Wren by criticizing him for failing to clarify the notion of force: "Although Wren did not employ the word 'force' in his paper, the dynamics implicit in his theory involves serious ambiguities despite the fact that he was able to derive a correct solution of perfectly elastic impact. To what does 'force' refer?"



Ra : Sa :: S : R

According to the first part of Wren's law of nature, "Bodies R, S having their proper velocities retain them even after collision." In other words, when the initial velocities of two bodies are reciprocally proportional to the size of those bodies, after the bodies meet, they will separate with the same speed as they approached, although in a contrary direction. There is a symmetry before and after collision. Wren has defined "proper" velocities by the same proportion that defines a balance in equilibrium. He makes the equivalence between this kind of collision and the balance explicit: "For this reason the collision of bodies having their proper velocities is equivalent to a balance swinging about its center of gravity." We will come back to the notion of "swinging" in a moment.

"Improper" velocities of bodies are just those velocities that are not reciprocally proportional to the size of the bodies. Here again it is the notion of the balance and equilibrium that is the key to the second part of his law of nature: "Bodies R, S having improper velocities are by collision returned to equilibrium." Wren provides some clarification. If it is the case that the velocities of R, S are not reciprocally proportional to the sizes of the bodies R, S, then however much the velocity of R exceeds, and that of S fell short of, what they would be if they were in the "proper" proportion before the collision, that much is subtracted from R and added to S by the collision. He again makes the equivalence between this kind of collision and the balance explicit (albeit a different kind of balance): "the collision of bodies which have improper velocities is equivalent to a balance reciprocating upon two centers equidistant either side of the center of gravity: for the balance may be extended into a yoke when the need arises."

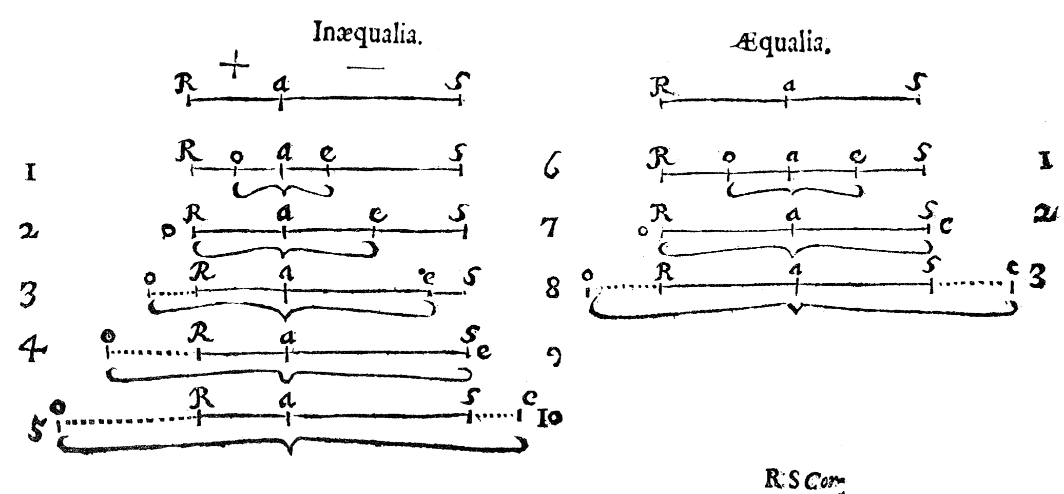
A notion of a balance reciprocating on two centers or "a yoke" becomes more apparent with Wren's diagrams. The diagrams themselves are a concise way to present the 13 different kinds of collisions of bodies with improper velocities. They are separated into two classes: the 5 diagrams on the left represent the 10 different ways *unequal* bodies move improperly; whereas the 3 diagrams on the right represent the 3 different ways *equal* bodies move properly. He defines his terms as follows:

> *R* and *S* are equal bodies; or *R* is the greater and *S* the lesser.
> *a* is the center of gravity or the fulcrum of the balance.
> *Z* is the sum of the velocities of the two bodies.

$\left.\begin{matrix} Re \\ Se \end{matrix}\right\}$ velocities of bodies $\left\{\begin{matrix} R \\ S \end{matrix}\right\}$ before collision (given)
$\left.\begin{matrix} OR \\ OS \end{matrix}\right\}$ velocities of bodies $\left\{\begin{matrix} R \\ S \end{matrix}\right\}$ after collision (sought)

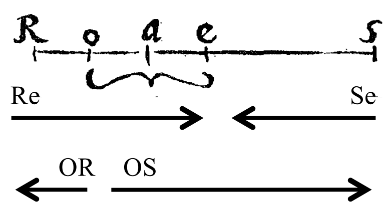
or

$\left.\begin{matrix} SO \\ RO \end{matrix}\right\}$ velocities of bodies $\left\{\begin{matrix} S \\ R \end{matrix}\right\}$ before collision (given)
$\left.\begin{matrix} eS \\ eR \end{matrix}\right\}$ velocities of bodies $\left\{\begin{matrix} S \\ R \end{matrix}\right\}$ after collision (sought)



If the velocities were "proper," the bodies would meet at *a*, the center of gravity. However, since they are "improper velocities," they exceed and fall short of the proper proportion and meet at *e* or *o* (depending on how the diagram is read—more on this in moment). So, for example, in the first instance, body *R* initially moves with velocity *Re*. As can be seen on the diagram, *Re* exceeds what would otherwise be its "proper" velocity *Ra*. Body *S* initially moves with velocity *Se*, and *Se* falls short of what would otherwise be its "proper" velocity *Sa*. The quantity which exceeded and fell short "is by the collision subtracted from *R* and added to *S* and vice versa." *Re* exceeds *Ra* by the quantity *ae*, just as *Se* fell short of *Sa* by *ea*. Through the action of the collision, a quantity equal to *ae* (the excess) is subtracted from what was the initial velocity of *R*, just as a quantity is added to what was the initial velocity of *S*. Thus, the final velocity of *R* is *OR,* and the final velocity of *S* is *OS*.

I



The first part of Wren's "Rule" makes this clear: "*Re, Se* becomes *OR, OS*."

The balance plays a number of roles in Wren's text: (1) "Proper" motion is defined as the reciprocal proportion of speeds and velocities, just like a balance in equilibrium; (2) the diagrams present the collision of bodies *R*, *S* as a balance, complete with a center of gravity and fulcrum at point *a*; (3) the action of collision itself (of both proper and improper velocities) as expressed by the law of nature is equivalent to a balance "swinging" about its center of gravity.

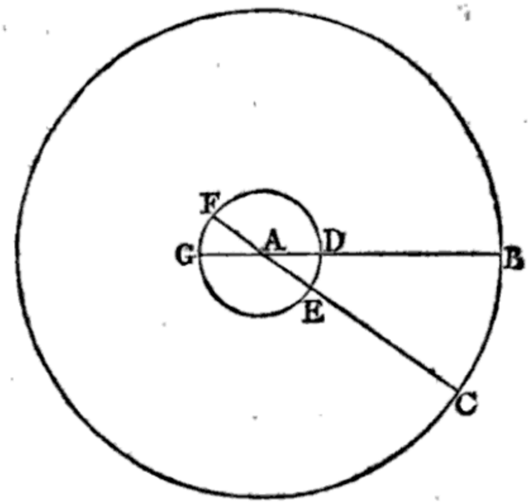
Wren's description of a "swinging" balance may be related to the pseudo-Aristotelian *Mechanical Problems* tradition of mechanics, which we have encountered in previous chapters (most notably that on Isaac Beeckman's mathematical work on collision), and which explains the reciprocal proportion between the length of the arms and the bodies by means of the speeds with which the bodies would move if the balance was rotated about its fulcrum.[644] The body on the shorter arm would move slower than the body at the end of the longer arm. This framework was then used (by people such as Beeckman) to relate the speeds and sizes of bodies in collisions. If not the pseudo-Aristotelian tradition explicitly, Wren's swinging balance may be related to the well-known reciprocal proportion between the weights or powers on the one hand and the motions of those weights or powers on the other hand, in many mechanical devices. One of Wren's mentors, John Wilkins, explains this very relationship in his chapter, "Concerning the proportion of slowness and swiftness in mechanical motions," which is the fifteenth chapter of his *Mathematical Magick*. Wilkins used the balance to illustrate the proportion between weights and powers to the speeds of the weights and powers in mechanical devices.

> For it is to be observed as a general rule, that the space of time or place, in which the weight is moved, in comparison to that in which the power doth move, is in the same proportion as they themselves are unto one another. So that if there be any great difference betwixt the strength of the weight and the power, the same kind of differences will there be in the spaces of their motion.[645]

[644] [pseudo] Aristotle, *Mechanical Problems*, in *Minor Works*, trans. by Walter Stanley Hett (Loeb Classical Library, Cambridge, Harvard University Press, 1936), 353. Problem 3 of the *Mechanical Problems* asks "Why is it that small forces can move big weights with a lever?" The weight which is moved, to the weight which moves it, is inversely proportional to the lengths of the arms of the lever. The reason for this, according to the author of the *Mechanical Problems* is due to speed. Think of two circles, a larger and smaller circle with the same center. Also imagine that both are drawn on the same disk. Now imagine a straight line drawn from the smaller circle, through the center and to the larger circle; at the intersections of this line and the circles are points. If one rotates the disk, the speed of the point on the larger circle will move faster than the speed of the point on the smaller circle. Now imagine that the center of the circle is a fulcrum and the radius of the smaller circle is the smaller arm of a balance, and the radius of the larger circle is the larger arm of a balance. The author of the *Mechanical Problems* claims that the speed of a body is inversely proportional to the weight. The longer arm of a balance will sweep out a larger arc of a circle than the short arm of a balance. Given the same force, a body at the end of the longer arm will move faster than that at the shorter arm.

[645] Wilkins, *Mathematicall Magick*, 147.

He provides a diagram and example:



> Let the line GAB represent a balance, or leaver; the weight being supposed at the point G, the fulciment at A, and the power sustaining the weight at B. Suppose the point G, unto which the weight is fastened, to be elevated unto F, and the opposite point B to be depressed unto C; it is evident that the arch, FG, or (which all one) DE, doth shew the space of the weight, and the arch BC the motion of the power. Now both these arches have the same proportion unto one another, as there is betwixt the weight and the power, or (which is all one) as there is betwixt their several distances from the fulciment. [...] And as the weight and power do thus differ in the spaces of their motions, so likewise in the slowness of it, the one moving the whole distance BC, in the same time wherein the other passes only GF.[646]

Wilkins points out that this is true no matter how great the "disproportions" are, and it is true for the mechanical motions in other devices, e.g. "pullies, wheels, &c."[647]

Wren uses the swinging balance to describe the speeds of bodies in collisions. The first part of Wren's law of motion states that "Bodies *R, S* having their proper velocities retain them even after collision." For Wren, collisions with "proper" velocities (*i.e.* the velocities and bodies are reciprocally proportional) are "equivalent to a balance swinging about its center of gravity." If a balance in equilibrium (*i.e.* a balance whose fulcrum is at its center of gravity) is tipped so that its arms oscillate (*i.e.* swing about its center of

[646] Ibid., 147-8
[647] Ibid., 148.

gravity), then the speed of the body on one of its arms (GA in the above figure) on its upswing (to FA) is the same as its speed on its downswing. Although the speed of the body at G would be less than the speed of the body at B, the speed of each body is the same for the subsequent oscillation, just as each body retains its speed after collision.

The second part of Wren's law of nature states that "Bodies R, S having improper velocities are by collision returned to equilibrium." These kinds of collision are "equivalent to a balance reciprocating upon two centers equidistant either side of the center of gravity: for the balance may be extended into a yoke when the need arises." In the diagrams the center of gravity *a* has been extended to "a yoke" represented by *oe*. Imagine the balance supported initially at *e*. Since this is not the center of gravity, the balance is not in equilibrium. The balance would rotate about *e*. This provides R and S with their initial velocities. Now that the balance is out of equilibrium, imagine another fulcrum placed at *o*. The balance would then swing back in the other direction, providing R and S with their final velocities. These two fulcrums are essentially an extended fulcrum (*i.e.* a yoke) about the center of gravity. Thus "reciprocating upon the two fulcrums, the balance is "returned to equilibrium." In the same way, "by collision" the bodies with their improper velocities are "returned to equilibrium."

Almost a decade before Huygens and Wren met in London to make predictions on the behavior of colliding pendulum bobs in 1661, Huygens had considered using—as an axiom in his theory of collision—a similar principle as that which would become the first part of Wren's law of nature. On a manuscript dating to 1652—the same manuscript which contains the algebraic equations studied in the previous chapter—Huygens wrote the following:

~~ax. 3~~. If a larger body A strikes a smaller body B, but the velocity of B is to the velocity of A reciprocally as the magnitude A to B [sic], then each will rebound with the same speed with which it came. If this is granted, everything can be demonstrated. Descartes is forced to grant it however.[648]

Indicative of the contrasting attitudes between Huygens and Wren on the proper justification of ideas (noted in a previous section of this chapter), Huygens also wrote: "But it must be seen whether it can be demonstrated from principles that are known better." Significantly, he also crossed out the name "ax. 3," and pursued other more fundamental principles on which to base his theory.[649] Huygens would later, in *De motu corporum ex percussione*, provide a rather complex demonstration of the proposition "~~ax. 3,~~" which in the later text became "Proposition VIII."[650]

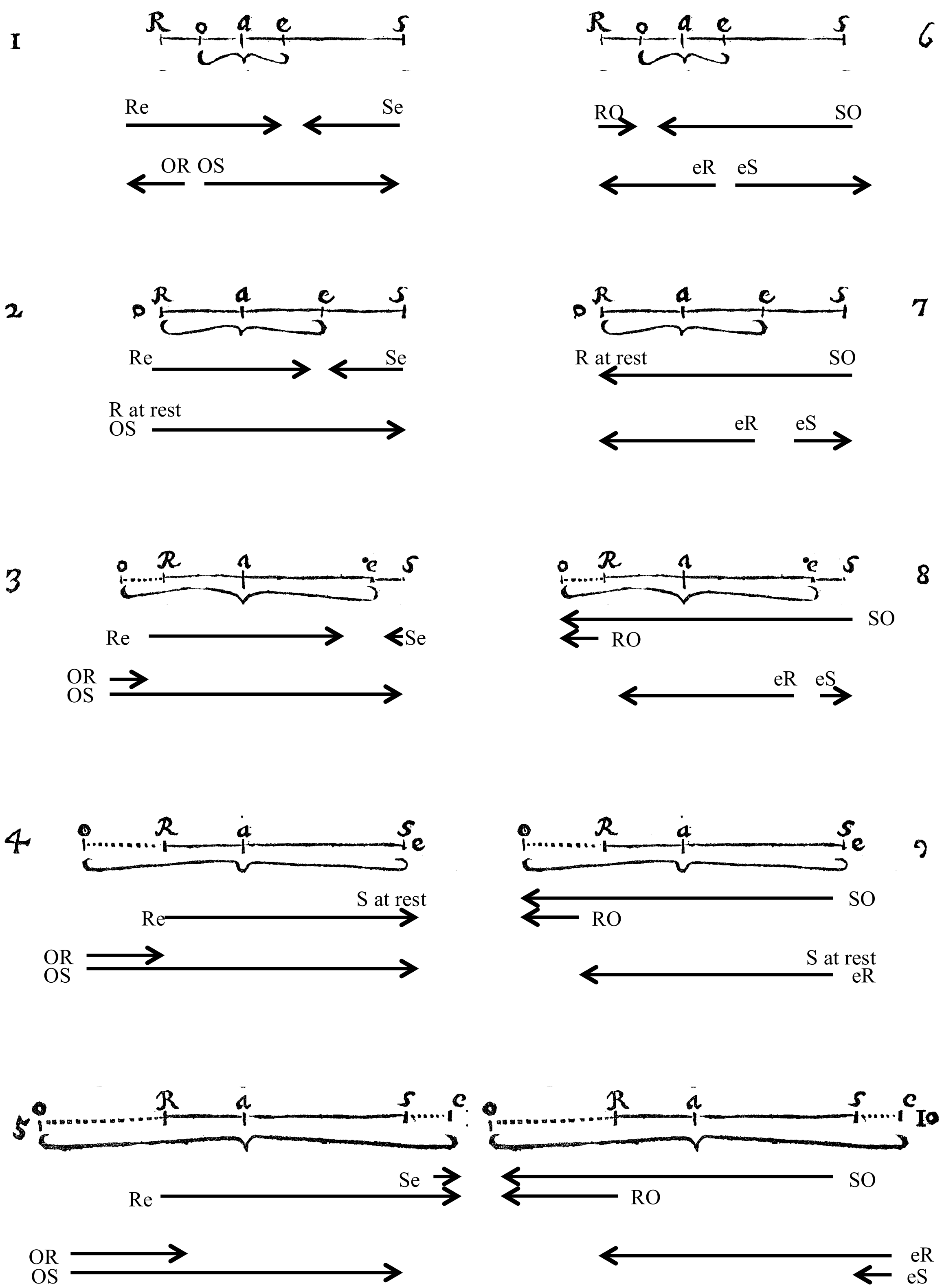
Wren, on the other hand, used the principle of the balance as the foundation of his law of nature. But it is important to note that Wren did not just use the notion of the balance as an analogy for collision. The proportions underlying the balance define his notions of proper and improper motion. And the relationships between bodies and speeds found in mechanical devices (such as the balance) provide a justification of the relationships between bodies and speeds in collision. In addition, the notion of equilibrium is at the heart of the action of collision. Moreover, the principle of a balance with two fulcrums proved to be a way to unify a vast array of combinations of differently sized bodies moving at different speeds and directions (or no speed at all). Using a single principle and a simple set of diagrams Wren could describe a set of combinations of two bodies meeting, which included several that his predecessors (notably Descartes) did not even consider.

[648] HOC 16: 96. Translation by Westfall, *Force in Newton's Physics*, 149.
[649] Westfall, *Force in Newton's Physics*, 150.
[650] HOC 16: 53-65.

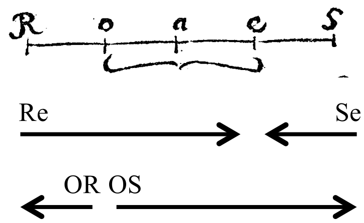
Wren's diagrams are worth considering further, both because they exemplify the unity and economy of his theory, but also because of the manner in which his diagrams present velocity as a quantity with both speed and direction.

As can be seen in the figure below, each of the diagrams on the left are numbered twice. Recall that body R is larger than body S in the five diagrams on the left. Note too that Wren provides two alternative definitions of his terms, in the left and right brackets respectively. Each of the five diagrams represents two different instances of collision. These have been separated in the table below.

Unequal Bodies



Equal Bodies



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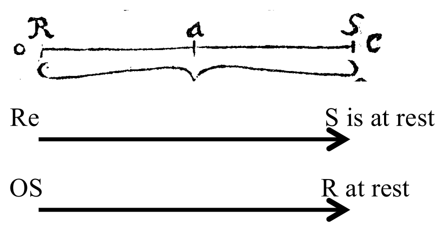

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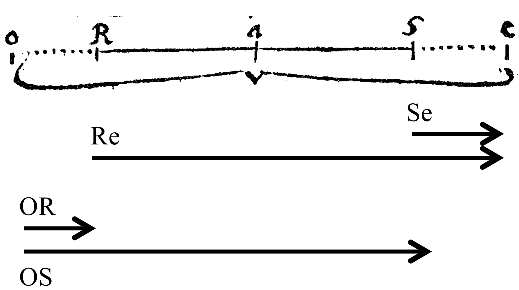

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In cases 1, 2, and 3 the larger body R and the smaller body S initially move toward each other. In the first, the bodies move away from each other after collision. In the third they move in the same direction after collision. The second provides the unique situation in which the larger body is left at rest after the bodies have collided and only the smaller body moves after the collision. In case 4 the larger body R meets the smaller body S at rest, after which they move in the same direction with S at a greater speed than R. And in case 5 both R and S initially move in the same direction, with the speed of R much greater than the speed of S. After the collision they continue moving in the same direction, but with S moving at a greater speed than R. Case 7 is notable, since it involves a smaller body S meeting a larger body R at rest. Unlike Descartes's 4th rule, the larger body moves after the collision and the smaller body rebounds in the contrary direction at a speed less than R.

The direction of the velocity is determined by the direction the symbols are read on the diagram. In cases 1 through 5 "*Re, Se* [the velocities of *R, S* before collision] become *OR, OS* [the velocities of R, S after collision]." In cases 6 through 10, *RO, SO* (the velocities of *R, S* before collision) become *eR, eS* (the velocities of R, S after collision). Wren provides the following "REGULA:"

> Read the syllables which are disjointed (*Re, Se, OR, OS,* or *RO, SO, eS, eR*) along the line for each way or type; those written in the diagram in the Hebraic way indicate a motion contrary to that denoted by any syllable in the Latin script. Joined syllables show that the body is a rest.

For example, in case 3, *Se* is read off the diagram "in the Hebraic way," whereas *Re* is read off the diagram in the "Latin way," which indicates that the motions are contrary. But, in case 5, both *Se* and *Re* are read off the diagram "in the Latin way" (*i.e.* the initial motions in this case are not contrary).

3.2.2 – Mathematics in the education of Wren: brevity, appeal to the eye, the analytic art

Christopher Wren was initially introduced to mathematics by William Holder (1616-1698), who had married Christopher's sister in 1643. Holder had known Seth Ward and Charles Scarburgh (1615-1693) from their time together studying at Cambridge, and it was through Holder that Wren likely met the "famous Physician" Dr. Scarburgh, to whom Wren would become an assistant, patient, and eventually colleague in the scholarly circles orbiting Wilkins at Wadham College Oxford, as well as the group at Gresham College, and ultimately the Royal Society.[651] In a letter to William Oughtred from the late 1640s, while Wren resided with Scarburgh, Wren wrote that "it is to his [Charles Scarburgh's] Kindness and Liberality of Mind that I am indebted not alone any little skill that I can boast in Mathematics, but for Life itself which, when suffering from recent sickness, I received from him as from the Hand of God."[652]

Charles Scarburgh himself had learned from William Oughtred. While at Cambridge, he and Seth Ward had been studying Oughtred's *Clavis mathematicae* (1631), an influential textbook in England on symbolic algebra. Meeting a passage they did not understand, Ward and Scarburgh traveled to Albury to visit "the renowned teacher" William Oughtred in person, and they became great friends.[653] Ward and

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[651] Helena M. Pycior, *Symbols, Impossible Numbers, and Geometric Entanglements: British Algebra through the Commentaries on Newton's* Universal Arithmetick (New York: Cambridge University Press, 1997), 42.
Mordechai Feingold, *The Mathematicians' apprenticeship: Science, universities and society in England 1560-1640* (New York: Cambridge University Press, 1984), 115. Bennett, *Mathematical Science of Wren*, 16. Lena Milman, *Sir Christopher Wren* (New York: Charles Scribner's Sons, 1908), 19.

[652] Milman, *Wren*, 21.

[653] Feingold, *Mathematicians' apprencticeship*, 89. Milman, *Wren*, 18. Florian Cajori, *William Oughtred: A Great Seventeenth-Century Teacher of Mathematics* (Chicago: Open Court, 1916), 60. Cajori quotes the account Anthony Wood gave of Ward and Scarborough's visit to "the country mathematician to be initiated into the mysteries of algebra." According to Wood: "[...] they took a journey to Mr. Will. Oughtred living

Scarburgh would go on to teach the symbolic "analytic art" to their own students. The above mentioned letter from Wren to Oughtred had been appended to a Latin translation that Wren had made of Oughtred's treatise on dialing, *The Golden Key*. Wren wrote the letter and translation on the urging of Scarburgh.[654] Not only does Wren's later *Lex naturae de collisione corporum* use the same notation and algebraic conventions found in Oughtred's works, we also find several characteristics of Wren's later work (particularly in his theory of motion in the *Lex naturae*) already highlighted in the letter to Oughtred from the 1640s as the very features for which Wren praised Oughtred:

> Welcome indeed (most gifted of Men) was the Shining of your Key upon the Sphere of Mathematics in this Age of ours, so that even the most learned have regarded it, nor undeservedly, as a Guiding Light since, led by Thee, they have been able safely and surely to cross the great stormy ocean of Algebra and so attained to other and unexplored Regions of Mathematics, [...]
>
> [Y]our words...need no adorning but sparkle by their very Brevity, that Brevity, I say, which to have attained is to have reached the very Summits of Literature; for very wisely in your Key you have rejected the Reasoning which is in common use among Men but which is useless in matters so abstruse; to this you have preferred symbols and figures which, without an Array of Words, enable the Reader to grasp your Meaning at a Glance. It is a hard method but for this very Hardness, to my thinking only more Divine, since it is an Imitating [sic] of those Celestial Beings who, unimpeded by Hindrances of Human Speech, by laying bare the soul, reveal all Mysteries.[655]

The "very Summits of Literature" is "Brevity." This is surely the literary ideal that Wren aimed for in his *Lex naturae.* It is through symbols and figures, and not "an array of words," that one can grasp "Meaning at a Glance."

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then at Albury in Surrey, to be informed in many things in his *Clavis mathematica* which seemed at that time very obscure to them. Mr. Oughtred treated them with great humanity, being very much pleased to see such ingenious young men apply themselves to these studies, and in short time he sent them away well satisfied in their desires. When they returned to Cambridge, they afterwards read the *Clav. Math.* to their pupils, which was the first time that book was read in the said university. Mr. Laur. Rook, a disciple of Oughtred, I think, and Mr. Ward's friend, did admirably well read in Gresham Coll. on the sixth chap. of the said book, which obtained him great repute from some and greater from Mr. Ward, who ever after had an especial favour for him."

[654] Pycior, *Symbols*, 42. Bennett, *Mathematical Science of Wren*, 16. Milman, *Wren*, 19-21.

[655] Milman, *Wren*, 20-2.

In 1647 Wren wrote to his father about his residence with Scarburgh and his translation of Oughtred's treatise on dialling. Scarburgh had convinced Wren that by making a translation of Oughtred's treatise he might win the favor of "all those Students of Mathematics who acknowledge Dr. Oughtred as their Father and Teacher."[656] Even in the letter to his father, Wren's esteem for brevity and economy of presentation comes to the fore:

> The other day I wrote a treatise on Trigonometry which sums up as I think, by a new method and in a few brief rules, the whole Theory of Spherical Trigonometry. An Epitome of this I re-wrote on a brass Disc of about the size of one of King James's Gold Pieces, and having snatched the Tool from the Engraver, I engraved much of it with my own Hand which Disc Sir Charles had no sooner seen than he insisted upon having a similar one of his own.[657]

Both "brevity" and "an appeal to the eye" were important to Oughtred. As Cajori has pointed out, "as compared with other contemporary works on algebra, Oughtred's distinguishes itself for the amount of symbolism used."[658] Indeed, another of Oughtred's students, John Wallis, would later write of Oughtred's methods in the fifteenth chapter of his *Treatise of Algebra* (1685) which was devoted to Oughtred's mathematical work:

> For though when Vieta first introduced this way of Specious Arithmetick, it was more necessary (the thing being new,) to express it in words at length: Yet when the thing was once received in practice, Mr. Oughtred (who affected by brevity, and to deliver what he taught as briefly as might be, and reduce all to a short view,) contented himself with single Letters instead of those words.[659]

In the preface to the English edition of the *Clavis mathematicae*, Oughtred indicated that his treatise was "not written in the usual syntheticall manner, nor with verbous

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[656] Ibid., 20.
[657] Ibid., 19-20. In the letter Wren also mentions that his "weather clock"—one of the mechanical inventions mentioned above—was described to Scarburgh. Upon hearing about it Scarburgh asked Wren to have it constructed in Brass at Scarburgh's expense.
[658] Cajori, *Oughtred*, 19.
[659] John Wallis, *A Treatise of Algebra, Both Historical and Practical, Shewing the original, progress, and advancement thereof, from time to time, and by what steps it hath attained to the heighth at which now it is* (London: John Playford for Richard Davis, 1685) 67-9. Quoted in Cajori, *Oughtred*, 33.

expreßions, but in the inventive way of Analitice, and with symboles or notes of things instead of words."[660] Oughtred acknowledged that many of his readers had found this to be "very hard" in the previous edition, but this was merely due to the "newnesse of the delivery." He then listed the strengths of his brief and symbolic approach, which includes "an appeal to the eye:"

> For this specious and symbolical manner, neither racketh the memory with multiplicity of words, nor chargeth the phantasie with comparing and laying things together; but plainly presenteth to the eye the whole course and process of every operation and argumentation.[661]

Or, as Wren would write in his letter to Oughtred, this manner was unhindered by human verbosity and was laid bare, so that the meaning could be known at a glance.

Oughtred wrote that his "key of mathematics" was the symbolic analytic art (a term by which he meant "taking the thing sought as knowne, we fine out what we seeke"[662]), which could be used to understand more easily the classics of mathematics:

> my Key was...to reach out to the ingenious lovers of these Sciences, as it were Ariadnes thread, to guide them through the intricate Labyrinth of these studies, and to direct them for the more easie and full understanding of the best and antientest Authors; such as are Euclides, Archimedes, Apollonius Pergaeus that Great Geometer, Diophantus Ptolomaeus, and the rest: That they may not only learn their propositions, which is the highest point of Art that most Students aime at; but also may perceive with what solertiousnesse, by what engines of aequations, Interpretations, Comparations, Reductions, and Disquisitions, those antient Worthies have beautified, enlarged, and first found out this most excellent Science.[663]

According to Oughtred, the symbolic expression of the classic texts allowed him to "more cleerly behold the things themselves," and to "uncas[e] the Propositions and

660 William Oughtred, *The key of the mathematicks new forged and filed together with a treatise of the resolution of all kinde of affected aequations in numbers*, 1st English edition, trans. Robert Woods (London: Tho. Harper for Rich. Witaker, 1647). The quoted passage comes from the first and second page of "To the Reader." The first Latin edition was published in 1631 as, "*Arithmeticae in numeris et speciebys institvtio: qvae tvm logisticae, tvm analyticae, atqve adeo totivs mathematicae, qvasi clavis est*."
661 Oughtred, *The key of the mathematicks*, ii.
662 Ibid., iv.
663 Ibid., ii-iii.

Demonstrations out of their covert words, designed them in notes and species appearing to the very eye."[664] In this form he could compare them, understand them, and "educe new out of them."[665] Among his works, Oughtred had "translated the tenth book of Euclid from its ponderous rhetorical form into that of brief symbolism."[666] Additionally, Cajori has noted that "in studying the ancient authors Oughtred is reported to have written down on the margin of the printed page some of the theorems and their proofs, expressed in the symbolic language of algebra."[667] This is precisely what Christopher Wren and Seth Ward would do with Galileo's *Discorsi e Dimostrazioni matematiche intorno à due nuove scienze* (1638), as Renée Raphael has shown in her 2014 article: "Galileo's *Discorsi* as a tool for the analytical art"[668]

John Wallis and Christopher Wren, and many other English mathematicians and natural philosophers such as Charles Scarburgh and Seth Ward were clearly influenced by Oughtred's *Clavis mathematicae.* Many used Oughtred's notation and conventions, such as capital letters from the beginning of the alphabet for given numbers, as well as the notation :: for proportion, and "St. Andrew's Cross" × for multiplication, the latter for which Oughtred is generally recognized as having introduced. Even practices that appear idiosyncratic to Oughtred, such as his manner of representing composite expressions, can

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[664] Ibid., iv.

[665] Ibid., ii-v. Oughtred provides a nice description of "analysis" as understood at the time: "Lastly, by framing like questions problematically, and in way of Analysis, as if they were already done, resolving them into their principles, I sought out reasons and means whereby they might be effected. And by this course of practices, not without long time, and much industry, I found out this way for the helpe and facilitation of Art."

[666] Cajori, *Oughtred*, 28.

[667] Ibid., 85.

[668] Renée Jennifer Raphael, "Galileo's *Discorsi* as a tool for the analytical art," *Annals of Science* (2014): 1-25. Accessed January 25, 2015. http://dx.doi.org/10.1080/00033790.2014.894850.

be found in Wren's *Lex naturae*.[669] Oughtred also recognized the double function of the signs $+$ and $-$ as both indicative of the "quality of numbers" as well as the operations of addition and subtraction.[670] And most importantly, Wallis, Ward, and Wren used the "analytic art."[671] Rather than "synthetically" demonstrate a result from prior principles, Wren, for example, found it "analytically." In other words, he treated what is "sought" as if it were known by representing it speciously with a symbol or note, and by determining its relationships to what is known, he could "find out what we seeke." Recall that Wren explicitly designated his symbols for final velocities as "sought" and initial velocities as "given." Raphael has shown that both Seth Ward and Christopher Wren rewrote Galileo's synthetic geometrical demonstrations into the language of symbolic algebra in order to practice and perhaps to teach the "analytic art." She argues that these annotations provide evidence of a pedagogical relationship between Ward and Wren. The latter seems to have copied the marginal symbolic algebra in which Ward had re-written Galileo's demonstrations. She has claimed, persuasively, that Galileo's *Discorsi* was a tool for "Ward and Wren ... to practice and teach their own analytical techniques."[672]

William Oughtred, both directly and through his students Charles Scarburgh and Seth Ward, had an influence on Christopher Wren's understanding and expression of mathematics. This can be seen directly in their shared passion for brevity of expression, and the symbolic presentation of mathematics which lays it bare to the eye so that it can be known at a glance rather than mediated through language and a taxed imagination.

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[669] The sum of two quantities is represented by the last letter of the alphabet: $A + E = Z$ for Oughtred. He uses this convention, for example, in his solution of the quadratic equation. Recall that Wren represents the sum of the speeds before collision by *Z*. See Cajori, *Oughtred*, 27.
[670] Cajori, *Oughtred*, 25. Pycior, *Symbols*.
[671] Technically, Wallis disconnected algebra from the analytic art, if by "analytic art" the analytic method in geometry is implied. For Wallis, the foundations of algebra were not to be found in geometry. In his understanding, algebra is more akin to an abstraction of arithmetic. See Pycior, *Symbols*.
[672] Raphael, "*Discorsi* as a tool," 4.

And it can be seen in the use of "the analytic art" as a means to solve problems by "taking the thing sought as knowne, we finde out what we seeke."

Wren may have been influenced by the practical mathematics tradition in London, as we have seen Bennett strongly suggesting in section 2 above. And Wren may well have been familiar with John Wilkins's *Mathematical Magick* which attempted to persuade scholars of the worthiness of practical mathematics. However, it is notable that Oughtred, who was clearly an important source of Wren's ideas on mathematics, distanced his "analytic art" from practical mathematics. Oughtred wrote in the preface of the first edition of his *Clavis mathematicae* (1631) that his symbolic analytic art reveals "the height and depth of mathematical science." This is in sharp contrast to the practical mathematicians and their tools about whom he wrote:

> I ignore the would-be mathematicians who occupy themselves only with the so-called practice, which is in reality mere juggler's tricks with instruments, the surface so to speak, pursued with a disregard of the great art, a contemptible picture.[673]

The symbolic analytic art is not an instrument that remains on the practical "surface" of mathematics, nor is it merely a tool for easy calculation according to Oughtred. Rather, it penetrates into the essence of mathematics, laying it bare.

One of the self described purposes of Oughtred's *Clavis mathematicae* was to serve as "Ariadne's thread" for students of the classics of mathematics. Oughtred himself was in the practice of rewriting classical demonstrations using his analytic art. This was not a "juggler's trick" to more easily (but superficially) solve a problem. It was meant to penetrate into the mathematics itself. Ward continued this method with his "analytic" study and rewriting of Galileo's *Discorsi*. Wren too, perhaps following the lead of

[673] Translation by Cajori, *Oughtred*, 20.

Oughtred and Ward, used the analytic art to reveal the height and depth of Galileo's mathematical science. And Wren seems to have extended this method of using Oughtred's "analytic art" to lay bare the meaning of mathematical expressions to another book written in the language of mathematics—the "book of nature" described in Galileo's *Assayer*:

> *Philosophy [i.e. physics] is written in this grand book — I mean the universe — which stands continually open to our gaze, but it cannot be understood unless one first learns to comprehend the language and interpret the characters in which it is written. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it; without these, one is wandering around in a dark labyrinth.*[674]

Christopher Wren was incredibly sparing in his use of language in his *Lex naturae de collisione corporum*. Nevertheless, after his sets of symbolic equations he closed his paper with a single sentence: "Nature obeys the algebraic laws of addition and subtraction." The thread that Theseus follows out of the Minotaur's labyrinth is the analytic art. Symbolic algebra lays bare the operations of nature itself.

3.2.3 – Smaller than nothing: negative numbers in early English algebra

For mathematicians in the 17th century it was difficult to accept that a magnitude could be smaller than nothing.[675] Accepting such a thing would have defied notions fundamental to mathematics at the time. "Nothing" was thought to be the limit for how small a quantity could be. Nevertheless, those working with algebra—whether it was understood to be the "analytic method" in geometry,[676] or an abstraction of arithmetic—

[674] Galileo Galilei, *Il Saggiatore* (Rome, 1623). Translation by Stillman Drake, *Discoveries and Opinions of Galileo* (New York: Anchor Books, 1957), 237-8.
[675] In this section I closely follow Pycior, *Symbols*. Also see section 3.1.4 above.
[676] AT VI 367-485. *La Géométrie*.

found that the system produced what seemed to be negative quantities: "quantities less than nothing." The response of some algebraists was to simply ignore negative roots of polynomials, others ambivalently called them "defective," "deficient," or "absurd," and several drew a compromising distinction between "species" marked by a negative sign such as -2A which were acceptable in mathematics, and negative *numbers*, such as -2 which were thought to be impossible and absurd. In short, the system produced results that were outside of what seemed to be the accepted boundaries of mathematics. This posed a problem for the proper interpretation of algebra and its relationship to the rest of mathematics. There were two general strategies for resolving the problem: (1) enforce the boundaries of mathematics and reject those results that fall outside of the accepted canons of mathematics. Or (2) the seemingly impossible results (such as negative quantities) should be accepted and the longstanding rules of mathematics should be rewritten instead.

In hindsight, the second route is obvious, and the acceptance of negative and "imaginary" numbers was just the first in an ever expanding mathematical universe of numbers and concepts. But, for mathematicians in the 17th century, this choice was not obvious and there was much disagreement. The second route was, after all, the much more radical approach: overturn the established rules of mathematics to make room for anomalies that seemed to defy common sense. And those who at the time chose the second route did not (or could not) establish the legitimacy of "negatives" and "imaginaries" with arguments internal to mathematics. John Wallis in his *Treatise of Algebra*, for example, attempted to legitimize negative and complex numbers not with mathematical or metaphysical arguments, but rather by appealing to *precedent*, *analogy*, and *usefulness*.[677] He reinterpreted the work of previous English algebraists such as William Oughtred and Thomas Harriot to show that *they* had accepted negative quantities, therefore he could too. According to modern historians, it is not at all clear that his predecessors did. Even Wallis—17th century champion of the negative number—acknowledged that strictly speaking it is impossible to have a quantity less than nothing and a number fewer than none. But, if one "supposed" that it was not impossible, one finds that these "fictional," "imaginary," impossible quantities were *useful*—particularly in physical applications.[678] One of the foremost physical applications to which Wallis appealed to provide a justification of these impossible quantities was the direction of motion.

Wallis was promoting this strategy at roughly the same time that the interpretation of negative quantities were proving to be problematic for Huygens's heuristic use of algebra to study collision (examined in the previous chapter). So, what we have is a coincidence between collision and algebra. Negative numbers made sense of collision (especially conservation principles) while at the same time, contrary motion (which is intrinsic to any study of collision) made sense of negative numbers.

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[677] Pycior, *Symbols*, 128.

[678] John Wallis, *Mathesis universalis, seu opus arithmeticum* (1657), in *Opera mathematica*, vol. 1, 11-228 (Oxoniae: E. Theatro Sheldoniano, 1695), 69-70. "Yet, although this is impossible, mathematicians and especially algebraists, look upon it as though it were not impossible. For they suppose, besides real quantities, certain *imaginary* quantities which are less than nothing. [...] Nor is this supposition absurd. For when they say, 5-8=-3, it is as though they said: He who supposes 8 to be subtracted from 5 supposes a certain third number less than 0." Translation by Scott, *Mathematical Work of Wallis*, 68-9. Also see Pycior, *Symbols*, 129.

3.2.4 – The Algebra of Nature

Wren's compact algebraic equations are presented as a "calculus" for numerically determining the velocities of R and S after collision. With some unpacking we will see that Wren's use of algebra is significantly different from that found in Huygens's manuscripts. Huygens must have used his algebraic equations as a calculus for predicting the results of the experiments in 1661, which is where Wren may well have been inspired to do the same. However, Huygens's algebra (in the tradition of Descartes) was used primarily in private and as a heuristic tool in his manuscripts, whereas Wren's algebra (in the tradition of Oughtred) is published and captures the essence of his law of nature. A particularly important difference in the algebraic physico-mathematics is the manner in which direction is indicated. Recall from the previous chapter that Huygens derived two nearly identical sets of equations to avoid producing negative quantities when determining the speeds of bodies after collision. Wren on the other hand provides a unified system of equations that accommodates any combination of two bodies colliding, rather than the single set of scenarios in which one body is at rest as was the case for Huygens. And, significantly, Wren's equations uses the negative sign to indicate direction.

Wren's algebra is presented as follows:

$$R+S : S :: Z : Ra \qquad Re - 2Ra = OR \qquad SO - 2Sa = eS$$

$$R+S : R :: Z : Sa \qquad 2Sa \pm Se = OS \qquad 2Ra + RO = eR$$

R and *S* are equal bodies; or *R* is the greater and *S* the lesser.
a is the center of gravity or the fulcrum of the balance.
Z is the sum of the velocities of the two bodies.

$$\left.\begin{array}{l} \left.\begin{array}{l}Re\\ Se\end{array}\right\}\text{velocities of bodies}\left\{\begin{array}{l}R\\ S\end{array}\right\}\begin{array}{l}\text{before collision}\\ \text{(given)}\end{array} \\[2ex] \left.\begin{array}{l}OR\\ OS\end{array}\right\}\text{velocities of bodies}\left\{\begin{array}{l}R\\ S\end{array}\right\}\begin{array}{l}\text{after collision}\\ \text{(sought)}\end{array} \end{array}\right\}\text{ or }\left\{\begin{array}{l} \left.\begin{array}{l}SO\\ RO\end{array}\right\}\text{velocities of bodies}\left\{\begin{array}{l}S\\ R\end{array}\right\}\begin{array}{l}\text{before collision}\\ \text{(given)}\end{array} \\[2ex] \left.\begin{array}{l}eS\\ eR\end{array}\right\}\text{velocities of bodies}\left\{\begin{array}{l}S\\ R\end{array}\right\}\begin{array}{l}\text{after collision}\\ \text{(sought)}\end{array} \end{array}\right.$$

The first set of relations is a proportion rather than equation. They are closely related to the reciprocal proportions in Wren's definition of "proper" motion. However, there is a significant difference, which will be explained below. The middle set of equations solve for OR and OS, which are the velocities of bodies R and S after collision in cases 1 through 5. The right most set of equations solve for eS and eR, which are the velocities of bodies R and S after collision in cases 6 through 10. The middle and right sets of equations are closely related to the explanation of Wren's Law of Motion, namely that however much the velocity of R exceeds its proper velocity and S falls short, that much is subtracted from R and added to S and vice versa. In addition to his "Latin" and "Hebraic" ways of indicating the direction of motion, Wren uses the algebraic signs for addition and subtraction to indicate direction. However, Wren's use of signs is different from Huygens's. This will become more clear below.

As written, the proportions and equations are very compact. There is a symmetric elegance to their presentation. The latter each involve only three terms. The equations for the velocities after collision also *appear* to be solely in terms of other velocities—the velocity before collision and the "proper" velocity before collision. However, this is masking a rather more complex set of relations. As Wren himself makes clear in his definition of terms, the speeds before collision (*Re, Se*) are "given," and the speeds after collision (*OR, OS*) are "sought." The numerical values for *Ra* or *Sa,* however, are not immediately obvious. In an experimental setting, if the bodies and velocities are not reciprocally proportional, *Ra* and *Sa* would not be given. In other words, if the colliding bodies do not have "proper velocities," *Ra* and *Sa* would not be directly known. But they can be calculated. This is where the set of proportions on the left become relevant.

To calculate the speed of *R* after collision (*OR* for example: $Re - 2Ra = OR$), the value for *Ra* is needed. Just such a value can be calculated using the proportion, $R{+}S : S :: Z : Ra$. This puts *OR* in terms of both the initial speed *Re* as well as the bodies *R, S*.

It is worth pausing for a moment to consider the proportion:

$$R{+}S : S :: Z : Ra \qquad \text{(pr. 1)}$$

Recall that "*Z* is the sum of the velocities of the two bodies." As has been mentioned, this proportion is very nearly the reciprocal proportion that defines "proper" velocity. Written in Wren's symbolic convention the reciprocal proportion defining "proper" velocity would be the following:

$$R : S :: Sa : Ra \qquad \text{(pr. 2)}$$

"Pr. 2," however, would not be very helpful for calculating *Ra*, since *Sa* is also not given.[679]

To calculate *Ra*, it appears that "pr. 1" would be converted from a proportion to an equation of quotients. There was disagreement about the legitimacy of a numerical interpretation of proportions at the time of Wren's work. Isaac Barrow and Thomas Hobbes, for example, rejected the understanding of ratios as quotients in favor of the classical conception. But several of Wren's peers and predecessors were advocates of this operation and the numerical conception of ratios, namely John Wallis and William Oughtred.[680] Solving for *Ra*:

$$Ra = Z\left(\frac{S}{R+S}\right) \qquad \text{(eq. 1)}$$

The equation for the velocity of *R* after collision,

$$Re - 2Ra = OR \qquad \text{(eq. 2)}$$

then becomes:

$$Re - \frac{2S(Z)}{R+S} = OR \qquad \text{(eq. 3)}$$

With a few simple algebraic steps, this equation can be written as follows:

$$\left(\frac{R-S}{R+S}\right) Re + \left(\frac{2S}{R+S}\right) Se = OR \qquad \text{(eq. 4)}$$

[679] It would appear that the reciprocal proportions provided by Wren in his "calculus" may have been "derived" from the "proper" velocity proportions. R:S::Sa:Ra => R+S:S::Sa+Ra:Ra. And, although not demonstrated, it at least seems intuitively obvious that Sa+Ra = Se+Re. Thus, we have R+S:S::Z:Ra. The reciprocal proportion for Sa can be "derived" similarly.

[680] Doug Jesseph, *Berkeley's Philosophy of Mathematics* (Chicago: The University of Chicago Press, 1993), 148-53. Katherine Hill, "Neither Ancient nor Modern: Wallis and Barrow on the Composition of Continua. Part One: Mathematical Styles and the Composition of Continua," *Notes and Records of the Royal Society of London* 50 (1996): 165-78. Katherine Hill, "Neither Ancient nor Modern: Wallis and Barrow on the Composition of Continua. Part Two: The Seventeenth-Century Context: The Struggle between Ancient and Modern," *Notes and Records of the Royal Society of London* 51 (1997): 13-22.

It is notable that this equation is nearly identical in form to the modern equation for the final velocity of a body (eq. 7) after elastic collision, derived from the equations for the conservation of momentum (eq. 5) and kinetic energy (eq. 6).

$$m_1 v_{1i} + m_2 v_{2i} = m_1 v_{1f} + m_2 v_{2f} \qquad \text{(eq.5)}$$

$$\frac{1}{2} m_1 {v_{1i}}^2 + \frac{1}{2} m_2 {v_{2i}}^2 = \frac{1}{2} m_1 {v_{1f}}^2 + \frac{1}{2} m_2 {v_{2f}}^2 \qquad \text{(eq. 6)}$$

$$\left(\frac{m_1 - m_2}{m_1 + m_2}\right) v_{1i} + \left(\frac{2m_2}{m_1 + m_2}\right) v_{2i} = v_{1f} \qquad \text{(eq. 7)}$$

$$\left(\frac{2m_1}{m_1 + m_2}\right) v_{1i} + \left(\frac{m_2 - m_2}{m_1 + m_2}\right) v_{2i} = v_{2f} \qquad \text{(eq. 8)}$$

Similarly, using the lower proportion, R+S : R :: Z : Sa (pr. 3), in the left set to solve for *Sa*,

$$Sa = Z\left(\frac{R}{R+S}\right) \qquad \text{(eq. 9)}$$

and the lower equation in the middle set, which is solved for the speed of *S* after collision,

$$2Sa \pm Se = OS \qquad \text{(eq. 10)}$$

the following equation can be produced, which is entirely in terms known before the collision, and which is nearly identical in form to the modern equation for the final velocity of the second body (eq. 8).

$$\left(\frac{2S}{R+S}\right) Re + \left(\frac{S-R}{R+S}\right) Se = OS \qquad \text{(eq. 11)}$$

Recall from the previous chapter, that the equations in Huygens's manuscripts described collisions in which one of the bodies was initially at rest. His equations were similar to "eq. 11" if *Se*=0 (or, using modern notation, "eq. 8" if v_{2f}=0). Significantly, Huygens derived equations similar to "eq. 11" (when *Se*=0) twice, and conspicuously avoided deriving anything that corresponds to "eq. 4" when *Se*=0 (or, using modern notation, "eq. 7 when v_{2f}=0). We noticed that "eq. 4" when *Se*=0 ("eq. 7 when v_{2f}=0) can provide negative values for the speed of the first body after collision. But "eq. 11" when *Se=0 cannot* produce negative values (as long as positive values for the sizes of the bodies and the initial speed). This and other evidence from Huygens's manuscripts strongly suggests that Huygens deliberately avoided negative quantities.

Not only can Wren's equations accommodate two bodies initially in motion rather than just one, Wren appears to have had a significantly different attitude regarding negative quantities.

Wren's acceptance and use of negative quantities to indicate the direction of velocities is apparent even in the simple presentation of the equations in his paper, particularly the equation for the velocity of *R* after collision (eq. 2). As we have seen, if the velocities are proper, then *Re*=*Ra* and *Se*=*Sa*. Wren's law of nature states that bodies having proper velocities retain them after collision, but move in a contrary direction. This is just what the equations show, making use of a negative algebraic sign: Re − 2Ra = OR. Since the velocity is proper, *Re*=*Ra*. So, substituting *Ra* for *Re*, the equation becomes: Ra − 2Ra = OR, which reduces to −Ra = OR.

Additionally, the meaning of the term Z, which Wren defines as the sum of the velocities of the two bodies, relies on the use of the negative sign to indicate motion in a contrary direction. Rather than the equation $Z = Re + Se,$ Wren seems to use the equation $Z = Re + -Se$, or more simply $Z = Re - Se$. Using the convention that positive velocity is in the "Latin" direction on the diagrams, then *Re* is a positive quantity in cases 1-5. Those velocities read in the "Hebraic" direction are contrary and thus negative quantities.

However, Wren's formalism is not without its ambiguities. There is the potential to confuse the "Hebraic/Latin" convention with the algebraic convention. Complicating matters more, Wren appears to use the symbol ± to indicate that in some cases a quantity will be added and in some it will be subtracted, but in his spare style he does not specify when. Presumably this matter is decided in conjunction with the use of the diagrams of the cases to determine whether the relevant velocities are contrary or not.[681]

Nevertheless, Wren gives the equations an elegant, symmetric presentation. In these brief symbolic equations, the relevant relationships between the physical quantities involved in collision are "known at a glance." As Wren himself may have explained (if he did not adhere so closely to his literary style of "brevity"): the nature of collision, which is fundamental to an understanding of change in the natural world, is laid bare by the symbolic equations. And unlike Huygens's equations, which may have been his inspiration in 1661, Wren's equations encompass every kind of collision of two bodies and do so by embracing the *usefulness* of "impossible" negative numbers.

Section 4
Conclusion

The members of the Royal Society attempted to determine the laws of motion with their new "experimental philosophy." Wren, who was experienced in the tradition of practical mathematics and mathematical instruments, was responsible for producing an instrument, composed of colliding pendulum bobs, for the purpose of performing

[681] Making matters worse, the symbol only appears in the lower equation of the middle set of equations in the edition of Wren's paper printed in the *Philosophical Transactions*. The paper reproduced in the OCH also includes the symbol for the corresponding equation in the set on the right.

experiments on collision. The members of the society recognized him as an authority on the topic. It was back to Wren's ideas that they referred, upon the completion of the series of experiments in both 1666 and 1668. These experiments had raised several questions, such as the status of Descartes's rules (the 6th rule was in obvious conflict with their experiments), the nature of motion (the relationship between speed and direction, and whether motion can be created or destroyed), and the relationship between elasticity and rebound. Careful experimentation with mathematical instruments was a new way to investigate collision. The purpose of these investigations was not just to understand the collision of wooden pendulum bobs. The members of the Royal Society were interested in understanding the nature of body and motion, and the fundamental constituents of nature. Many held out the hope that the interaction of these small bodies would explain "*Generation, Corruption, Alteration, and all the Vicissitudes of Nature.*"

After nearly a decade of experiments, Huygens, Wren, and Wallis published their rules in the *Philosophical Transactions.* However, the published theories were not driven purely by experiments. They were not simply lists of experimental data. None of the papers makes any mention of the experiments. Rather, the theories have their own internal mathematical justifications. The proportions of the balance provide the quantitative relations for both Wallis and Wren. Wallis focused on the powers required to move a body on the model of the lever, whereas Wren emphasized the equilibrium of the balance. Wallis expresses these proportions with his "*specious arithmetic*" and Wren does so with economical use of diagrams of balances organized according to principles of symmetry and a concise set of algebraic equations.

Wallis's theory is not governed by prior conservation principles. It does, however, rely on a contest between *impetus* and *impedimentum*, similar to the Cartesian contest between moving force and force or resistance, or Marci's contest between impulse and resistance. Unlike Descartes or Marci, the contest does not explain rebound for Wallis. According to Descartes, if the moving force is not greater than the force of resistance, no motion will be transferred between bodies, and the moving body will rebound with no loss of motion. For Wallis, on the other hand, if the *impetus* does not overcome the *impedimentum*, the body stops. For Descartes, the conditions of rebound were determined by the contest view of force and the Cartesian conservation of quantity of motion. The cause of rebound for Wallis is "springyness" (and the force of restitution such springiness necessitates). According to Wallis, a perfectly hard body is not elastic. As we saw last chapter, Huygens's hard bodies behave as if they were perfectly elastic, but he does not provide a rationale. The bodies Wren describes also behave as if they were perfectly elastic, but he does not explicitly state whether they are hard, soft, or elastic. In addition to categorizing the nature of bodies, Wallis (unlike Wren and Huygens) also engaged the topic of transdiction between the colliding pendulum bobs and the interaction of minute bodies.

The equilibrium of the balance plays several roles in Wren's theory. "Proper" motion is defined as the reciprocal proportion of speeds and velocities, just like a balance in equilibrium. If the sizes of the bodies are reciprocally proportional to the speeds of the bodies, each will rebound with the speed with which it approached. The diagrams present the collision of bodies *R*, *S* as a balance, complete with a center of gravity and fulcrum at point *a*. And the action of collision itself (of both proper and improper velocities) as expressed by the law of nature is equivalent to a balance "swinging" about its center of gravity, extended over a yoke. The principle of the balance was the foundation of Wren's law of nature. But it is important to note that Wren did not just use the notion of the balance as an analogy for collision. The proportions underlying the balance define his notions of proper and improper motion. The principle of a balance with two fulcrums proved to be a way to unify a vast array of combinations of differently sized bodies moving at different speeds and directions (or no speed at all). Using a single principle and a simple set of diagrams Wren could describe a set of combinations of two bodies meeting, which included several that his predecessors (notably Descartes) did not even consider.

Huygens used Cartesian symbolic algebra as a tool to criticize Descartes's rules, and as scaffolding for a new principle, and as a way to predict the outcomes of colliding pendulum bobs before Wren and Wallis, who witnessed the prediction, could not at the time accomplish such an achievement. Huygens's algebra remained as a heuristic tool in his manuscripts. Wren and Wallis, on the other hand, were the first to publish rules of collision expressed in algebraic mathematics.

Wallis argued for an arithmetic understanding of algebra (a *specious arithmetic*), and attempted to legitimize both negative quantities and the roots of negative quantities. This was done, in part, by an appeal to their usefulness in physical applications. Wallis presents the directionality of motion and *impetus* with + and − signs. In so doing he provided a legitimation of an otherwise "impossible" quantity by an appeal to physical applications, and presented a new quantitative expression of contrary motion.

Both Wallis and Wren were influenced by the symbolic algebra of William Oughtred, however, Oughtred's values and style had a particular impact on Wren. The latter used not only his notation and terminology, but placed the same epistemological value on the economy of the symbol—so that meaning can be grasped at a glance. And, like Oughtred, he thought of the "analytic art" *not* as a practical mathematician's mere "juggler trick," but rather as a means to penetrate to the heart of mathematics. Oughtred used his analytic art to better understand the classical works of mathematics such as Euclid's *Elements*, and translated several of the problems into algebra. As Renée Raphael has shown, Oughtred's student, Seth Ward, taught Wren the analytic art by having him rewrite problems from Galileo's *Discorsi* in symbolic algebra. I have argued that Wren extended this tradition, and used Oughtred's symbolic algebra, which embraces brevity and the unity of expression made possible by a system of equations, to reveal the "height and depth" of the book of nature. Wallis and Wren's theories of collision mark a transition, previously carried forward by Huygens, in the "mathematization of nature."

Chapter 6
Conclusion

Philosophy [i.e. physics] is written in this grand book—I mean the universe—which stands continually open to our gaze, but it cannot be understood unless one first learns to comprehend the language and interpret the characters in which it is written. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it; without these, one is wandering around in a dark labyrinth.

—from Galileo's *Il Saggiatore*, 1623

Nature obeys the algebraic laws of addition and subtraction

—from Wren's *Lex naturae de collisione corporum*, 1669

CHAPTER 6 OUTLINE

This dissertation has been a historical narrative about development and change. The study of collision was of central importance in the changing explanations of nature in the 17th century. These explanations were part of the larger transformation, traditionally called the mathematization of nature. And mathematization itself was undergoing change, with the emergence of an *algebraic* physico-mathematics from a language whose characters were initially "circles, triangles, and geometric diagrams." Establishing the mathematical rules of collision was a new problem, and the views of the individuals who contested with it changed and developed. As we saw in chapter 2, Beeckman repeatedly returned to the topic throughout his life and offered multiple strategies (within the framework of the pseudo-Aristotelian balance) to describe various scenarios of colliding bodies. Descartes's views changed significantly, which was established in chapter 3, and in his later view of collision we have uncovered the classical analytic method for solving problems underlying the rules of collision. Huygens used multiple heuristic strategies, including Descartes's symbolic algebra, to criticize Descartes rules of collision and to develop his own principles before formulating his theory in axiomatic presentations, as shown in chapter 4. And the members of the Royal Society returned to experiments on collision over the span of almost a decade before publishing the rules and laws of motion of Wren, Wallis, and Huygens, as shown in chapter 5.

A commonality amidst this change was the importance of devices such as the pendulum, balance, and (less materially) symbolic algebra. Through the interaction with these objects, the investigators conceptualized and justified their mathematical theories of collision, and in doing so transformed systems of knowledge. The pendulum made possible a sophisticated form of empirical investigation. The manner in which collision could be empirically investigated transformed through the interaction with the pendulum. Central notions such as hardness and elasticity were reinterpreted through experiments with the pendulum, and several fundamental principles were likely developed on account of it. The balance was of key importance as both a framework for the conceptualization of collision and for the justification of the mathematical theories of collision. Several iterations of the balance—from Archimedean and pseudo-Aristotelian traditions, with emphases on equilibrium as well as the moving power of the lever—were significant in multiple facets of collision, including the epistemically privileged scenario in which the bodies and speeds are inversely proportional, the connection between equilibrium and conservation principles, as well as principles of symmetry. Algebra, although not a physical object that has heft when held in one's hands, was a device of sorts. Interactions with the apparatus of symbolic equations yielded surprising results that changed both the conceptualization of the directionality of motion as well as the notion of quantity. Symbolic algebra made possible a unified expression of collision in a system of equations with two unknowns, and became the mathematics of nature.

Section 1
Experiment

1.1 – Experience

All of the major figures featured in this dissertation, including Harriot, Beeckman, and Descartes, performed experiments. Harriot experimented with light and lenses, formulating the sine law of refraction as well as an explanation of the rainbow.[682] He also performed experiments to determine the motion of fall.[683] Beeckman experimented on the motion of fluids, as well as the speed of light.[684] Descartes responded to Beeckman's experiments on the speed of light. He also carried out his own famous experiments on the rainbow.[685] However, there is no evidence that any of these individuals performed experiments to develop or test their mathematical accounts of collision, as did Huygens, Wren, and Wallis. Common experience may have informed some of the principles, but in the cases of Beeckman and Descartes, they openly acknowledged that their theories of collision are in conflict with common experiences of colliding bodies. And, even in the Royal Society, some members were skeptical of the ability of experiments to provide insight on the fundamental motions of particles.

Beeckman advocated micro-mechanical explanations of natural phenomena. The causes of change in natural phenomena could be explained by the motions of corpuscles, which differed in size and shape, and which behaved like simple machines, *i.e*. according to the "mechanical principles of macro-phenomena."[686] At root, Beeckman held an atomist understanding of nature. However, his explanations tended to appeal to congeries of atoms. The former were perfectly hard, whereas the congeries could be compressed

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[682] J. A. Lohne, "Thomas Harriott (1560-1621) The Tycho Brahe of Optics," *Centaurus* 6 (1959): 113-121. J. A. Lohne, "Essays on Thomas Harriot," *Archive for History of Exact Sciences* 20 (1979): 189-312.

[683] Matthias Schemmel, *The English Galileo: Thomas Harriot's Work on Motion as an Example of Preclassical Mechanics*, 2 vols. (Berlin: Springer, 2008), 97-152.

[684] Spyros Sakellariadis, "Descartes' Experimental Proof of the Infinite Velocity of Light and Huygens' Rejoinder," *Archive for History of Exact Sciences* 26 (1982): 1-4. R. Hookyaas, "Beeckman, Isaac," *DSB* 1: 567.

[685] Jed Buchwald, "Descartes' Experimental Journey Past the Prism and Through the Invisible World to the Rainbow," *Annals of Science* 65 (2007): 1-46. Also see Daniel Garber, "Descartes and Experiment in the Discourse and Essays," in *Essays on the Philosophy and Science of René Descartes*, ed. by Stephen Voss (Oxford University Press, 1993), 288-310.

[686] Stephen Gaukroger and John Schuster, "The hydrostatic paradox and the origins of Cartesian dynamics," *Studies in History and Philosophy of Science* 33 (2002): 551.

and were thought to possibly offer some degree of restitution.[687] Regarding natural phenomena, collision was a fruitful *explanans.* Beeckman did not provide a mathematical account of the collision of congeries of atoms; his mathematical study of collision focuses solely on the collision of atoms. Collision as *explanandum* was problematic for Beeckman. Since atoms are perfectly hard (and do not compress) Beeckman thought they would not rebound. Throughout his mathematical studies of collision, to which he returned over several years in his *Journal*, Beeckman was primarily interested in mathematically describing the amount of *motion lost* with each collision of atoms. As Beeckman himself acknowledged, common experience clearly shows that phenomenal bodies do at times rebound, and motion in the world is not continually lost. Thus he thought that his own mathematical account of collision was in conflict with common experience. As a *deus ex machina* Beeckman suspected that God counteracts the loss of motion, shown by Beeckman's mathematical study of collision, by continually "enlivening" and thus conserving motion.[688]

Descartes fully acknowledged that the rules of collision found in his *Principles of Philosophy* conflict with common experience. He did not see this as a problem. Immediately after the rules are presented, the very next section of the French edition of the *Principles* begins with the words, "Indeed, experience often seems to contradict the rules I have just explained."[689] Descartes's rules describe the fundamental motions and interactions of bodies that are themselves the explanation of natural phenomena such as hardness, fluidity, light, and magnetism. The world Descartes describes is a plenum. Thus, the motion of any phenomenal body takes place in a fluid. The rules, however,

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[687] Beeckman, *Journal* 2:100-1; 3:31.
[688] See chapter 2.
[689] AT IX 93-4. *Principes* II 53.

being the simplest instances of bodies interacting, describe two hard bodies in isolation. Presumably, to accurately describe the motion of two *phenomenal* bodies colliding in the world, one would use the rules of collision to calculate the intervening motions of every body composing the fluid. But the purpose of the rules is not to accurately predict the outcome of experiments involving the collision of phenomenal objects such billiard balls. Rather, the rules provide an account of the fundamental motions and interactions of bodies in accordance with Descartes's conservation principle and laws of nature.[690]

Nevertheless, as we have seen in chapter 3, Descartes referred to a set of experiments and experiences that are connected to his theory of collision in a series of letters to Mersenne. In the letters Descartes refers to the comparative effect of hammers hitting bullets that have been positioned either on cushions or anvils. Descartes claimed that a hammer would immediately rebound off of the bullet placed on an anvil, whereas the hammer will flatten the bullet placed on a (plate, which has been placed on a) cushion. When met with incredulity by Mersenne, Descartes suggested how the experiment could be modified to produce the intended results more clearly. And he referred to the common experiences of chefs in the kitchens of Paris who more successfully break a shoulder of mutton if the mutton is held in the chef's hand or placed on a towel when struck, rather than placed on a hard table. Although never appearing in a treatise, Descartes referred to these experiments in his correspondence with Mersenne to provide support for a fundamental notion intrinsic to his impact law—Descartes's "contest view" of the force of resistance and the moving force. Unlike the experiments

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[690] Descartes had made much the same point in his early view as well. In his letter to Florimond de Beaune in 1639, he claimed that he has "never particularly examined questions that depend on measures of speed," because there is so much to consider that is more fundamental, *e.g.* the conservation of *quantity of motion*, before speed can be addressed. AT II 544. See chapter 3.

described in Descartes's letters, the experiments on collision at the Royal Society, not only provide illustrations and support for the theories—as the bullets, anvils, cushions and French chefs do for Descartes's contest view—but the experiments at the Royal Society were designed also to make controlled predictions to test various proposed theories of collision.

However, not all the members of the Royal Society were entirely convinced that experimentation was a legitimate means to investigate collision, if the purpose of a theory of collision was to understand the motions of the "minute particles" that explain all the vicissitudes of nature. Throughout Neile's correspondence with Wallis via Oldenburg, Neile is concerned with what has since come to be known as the problem of transdiction.[691] According to Neile both the nature of bodies (whether they are hard, soft, or elastic), as well as the ability of a body to offer resistance to another, are *apparent* in phenomenal bodies, but those qualities are explained by the *real* motion of minute particles. The minute particles may not share the same properties of hardness, softness, elasticity or resistance, as the bodies observed in experiments, since those properties are explained by the motion of minute particles, and since the minute particles are not observable. For Neile, this raised a methodological difficulty: how to determine the laws of motion of the fundamental components of reality, if one only has experimental access to the behavior of apparent bodies. Neile suggested that their only recourse was to investigate the unobservable minute particles by "reason" rather than by "experiment."[692] According to Neile, the study of phenomenal bodies, such as wooden pendulum bobs

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[691] Maurice Mandelbaum, *Philosophy, Science and Sense Perception: Historical and Critical Studies* (Baltimore: Johns Hopkins Press, 1964), 88-112. Neither Wallis nor Neile use the word "transdiction."
[692] OCH 5: 542. W. Neile to Oldenburg, 13 May 1669. "the foundations themselves I think are to be grounded upon reason for I doubt experiment will hardly ever cleare the nature of motion in minute particles."

colliding, in order to understand nothing more than the motion of wooden pendulum bobs, may be "good for use but it is not science or philosophye."[693] In the course of his discussion with Wallis, he eventually conceded, however, that any theory of motion (based on reason) should be consistent with the sets of experiments performed by the society.[694]

1.2 – Experiment

The pendulum was key to the experimental investigation of collision. But the rules of collision were not an obvious or easy consequence of the experimental use of the pendulum. The relationship between theory and experiment in the pendulum itself proved to be complex. There was an obvious enthusiasm for experimentation on collision in the early Royal Society, but this was tempered by some. Neile was ambivalent regarding the legitimacy of experiment to understand the motions of "minute bodies," and Huygens did not rely on experiment as grounds for justification of theory. Various individuals balanced the role of experiments differently, and as such, experimentation played many roles in the study of collision. Experiments served as the foundation of a theory of collision, and that by which theories were confirmed. They tested theories through predictions, but for some, experiments were only that with which a theory *accords* but not upon which the theory is *demonstrated*. Experiments were a source of discoveries, and they shaped fundamental concepts and principles.

According to Huygens, experiment was that to which theory is supposed to agree. The rules of collision, which are demonstrated axiomatically, should predict the results of

[693] OCH 5: 518. W. Neile to Oldenburg, 7 May 1669.
[694] OCH 5: 265, 312, 363, 542, 559. Neile extensively discusses "appearance and reality." See OCH: 286-7, 347, 364, 518, 542, for his discussions of "reason and experiment."

experiments. This is in contrast to Wren's mathematical theory of collision, which refrained from providing axiomatic demonstrations, as well as Marriotte's plan, in which experiments formed the foundation of his study of collision. While Huygens was responding to his French colleague Marriotte, whose experimental apparatus of colliding pendulum bobs and rules of impact were so similar to his own that Huygens accused Marriotte of plagiarism,[695] Huygens criticized what he considered to be Marriotte's overreliance on experimental justification. Whereas Huygens used principles such as the relativity of motion, the impossibility of perpetual motion, and symmetry to justify his rules of impact, Marriotte set experience as the foundation of his work.[696] Experiments, of course, had a place in Huygens's system. He showed, for example, that his rules accord with experiments, and he accurately predicted the results of colliding pendulum bobs in the first years of the Royal Society when no one else could. However, he did not think that experiment was a secure foundation for the rules of impact. Experiments were, in his words, "slippery."[697] In correspondence with Henry Oldenburg, the secretary of the Royal Society, Huygens also questioned the justification of Wren's theory of collision (although both he and Wren recognized that their rules of collision were in agreement with each other). Wren's theory did not appear to him to be suitably grounded on demonstration.[698] Wren responded by pointing out that any demonstration of his rules of collision would require the assumption of several other postulates, which would themselves require demonstration, and so on.[699] Although Wren's published theory of collision does not refer

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[695] Domenico Bertoloni Meli, *Thinking with Objects* (Baltimore: The Johns Hopkins University Press, 2006), 238.

[696] Bertoloni Meli, *Thinking with Objects*, 239.

[697] HOC 2: 114-5. Huygens to Sluse, 2 November 1657. "*Experientias me sectari ne extimes, scio enim lubricas esse*." Also see Bertoloni Meli, *Thinking with Objects*, 233.

[698] OCH 5: 360. Huygens to Oldenburg, 27 January 1668/9. Translation by Hall OCH 5: 362.

[699] OCH 5: 373. Oldenburg to Huygens, 4 February 1668/9. Translation by Hall, OCH 5: 375.

to experiments, Sprat in his *History of the Royal-Society of London* (1667) claimed that Wren's theory had been "*confirm'd by many hundreds of Experiments*" with the "instrument" that Wren had designed to study collision.[700]

In the context of the *meetings* of the Royal Society itself, experiments served as a means of testing the rules of collision through the predictions of the outcomes. Experiment was also a source of discoveries. Wren receives considerable credit as an authority in early experiments on collision. Likely, the rules of collision that would eventually be published in the *Philosophical Transactions* were developed through the discoveries made in the course of almost a decade of experiments.

Experiments and interactions with the pendulum also shaped the theoretical principles and concepts in the study of collision, such as Huygens's variant of the Torricelli principle, the principle of the reversibility of impact, and the conservation of "body times speed squared,"[701] as well as the notion of "hardness."[702] Notions of "hardness" and "elasticity" were, of course, prior to the study of collision, though reciprocally they were redefined by the reflections on collision. The notion of "hardness," for example, changed significantly, at times embracing both what we now call "perfectly inelastic" as well as "perfectly elastic bodies."[703] Huygens presumably started with some

[700] Thomas Sprat, *The History of the Royal Society of London* (London: T. R. for J. Martyn and J. Allestry, 1667), 312.
[701] See chapter 4, section 3.
[702] See chapter 4, section 4.1.
[703] The process by which this change took place is akin to what Hasok Chang has called epistemic iteration: "a process in which successive stages of knowledge, each building on the preceding one, are created in order to enhance the achievement of certain epistemic goals.... In each step, the later stage is based on the earlier stage, but cannot be deduced from it in any straightforward sense. Each line is based on the principle of respect and the imperative of progress, and the whole chain exhibits innovative progress within a continuous tradition." See Hasok Chang, *Inventing Temperature: Measurement and Scientific Progress* (New York: Oxford University Press, 2004), 226. Huygens's notion of hardness cannot be deduced from Descartes's, just as Descartes's cannot be deduced from Beeckman's. Descartes's changing understanding of collision from a view similar to Beeckman's to that which is expressed in the *Principles of Philosophy* has been documented in chapter 3. It is not clear whether Huygens intended to modify Descartes's notion of

common notion of hardness; experiments were performed with hard bodies such as wooden pendulum bobs; and the behavior of these wooden pendulum bobs then informed how hardness itself was understood, which in turn informed the validity of variously defined principles of collision, such as the conservation of body times speed squared.

Shapin and Schaffer's *Leviathan and the Air-Pump* drew attention to the importance of the technical, literary, and social practices that went into the production and justification of "matters of fact" in the Royal Society's experiments with the air-pump.[704] Although unmentioned in Shapin and Schaffer's study, the Royal Society had also been performing experiments on collision just prior to and contemporaneously with

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hardness. Although Huygens used his own notion of hardness in his arguments against Descartes, he did not explicitly indicate that it is a different notion from that of Descartes.

Beeckman understood simple atoms to be perfectly hard bodies. Since they are perfectly hard, Beeckman thought they could not compress, and thus there would be no rebound between the atoms themselves. Descartes contrasted hardness with fluidity, and explained both notions in terms of relative motion. A body is harder than another if its constituent parts are in less motion than those of another. A body is perfectly hard if its constituent parts are at rest with respect to each other. This provides a plausible explanation for the hardness of stone, wood, and other familiar objects. Complications arise, which have been discussed in chapter 3, when describing the interaction of simple bodies in abstraction, such as those in Descartes's rules of collision. The rebound of these simple hard bodies is explained, in Descartes's mature works, according to the contest model of force, which is found in his impact law. If the force of resistance in a body is not overcome, the body approaching it will rebound. If the force of resistance is overcome, motion will be transferred and the bodies will move together. Closely tied to his other fundamental notions of motion, rest, and the force of resistance, "hardness" is saddled with the same issues besetting the consistency of the fundamentals of his system. At any rate, Descartes's notion of hardness does not easily map onto later concepts of elastic and inelastic, nor was it developed in light of these concepts.

Huygens challenged Descartes's rules of collision in several ways, including a heuristic use of symbolic algebra and demonstrations involving principles of relativity and symmetry, but Huygens used neither Descartes's notion of hardness nor his notion of motion. I have not found evidence that he justified his new notion of hardness, nor that he indicated it as different from Descartes's. What seems to have taken place is a process of "enrichment" through "epistemic iteration." Starting with some common notion of hardness, experiments were performed with experimentally hard bodies such as wooden pendulum bobs. The behavior of these wooden pendulum bobs then informed how hardness itself was understood. And the meaning of hardness came to be re-conceptualized.

704 Steve Shapin and Simon Schaffer, *Leviathan and the Air-pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1989), 25.

the air-pump.[705] The main characters, Boyle and Hobbes, in the study of the air-pump are largely absent in the records of experiments on collision—although William Neile notably claimed that he followed Hobbes, when describing his theory of collision to John Wallis.[706] However, several of the other members of the Royal Society (and Shapin and Schaffer's study), such as Huygens, Wren, and Hooke, are of central importance. Yet, the thorny issues involved in the production of experimental matters of fact are perspicuously diminished or absent in the experiments on collision. The texts on collision produced by members of the society in no way resemble the style of "virtual witnessing" attributed to Boyle's exhaustively descriptive texts. There did not seem to have been the same fraught issues of replication of experiments in various times and locations as there were for the air-pump (*e.g.* Huygens's air-pump at the *Académie*). And, although the experiments were carried out by individuals and were subsequently performed at meetings, there does not appear to have been attention paid to enforcing the boundary of witnesses (as exemplified in the tense relationship between Boyle and Hobbes). William Neile, for example, who was sympathetic to Hobbes's matter theory, was a member of the Royal Society. He carried on a lively correspondence with Wallis on the topics of impact and motion, and presented his theory of collision to the Royal Society, which appears to have been received without policing.[707] Moreover, experiments on collision were carried on prior to the establishment of the Royal Society by circles of inquisitive individuals, and members

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[705] One exception is the following: "[I]n 1668, at the moment when [Huygens] presented his [air] pump to the Académie, he wrote a text entitled 'De gravitatione.' The presentation of this text at the Académie then elicited the violent debate between Huygens, Roberval, and Mariotte on impact laws and the character of subtle fluids that raged in the Académie during the autumn of 1669." Shapin and Schaffer, *Leviathan,* 270-1.

[706] OCH 5: 525. "This part of the theory I gladly acknowledge taking from the books of Mr. Hobbes."

[707] Thomas Birch, *The History of the Royal Society of London*, vol. 2 (London, A. Millar in the Strand, 1756), 361-2. OCH 5: 517-24. Neile's theory was presented to the Royal Society on 29 April 1669, and recorded as his "Hypothesis of Motion."

of the Society had an interest in the efforts and texts of other unaffiliated individuals. Admittedly "matters of fact" are in a different epistemic category than "rules of impact" or "laws of motion." Nevertheless, the plurality of strategies of justification (which do not align with those expressed in *Leviathan and the Air-Pump*) for the rules of collision in the Royal Society is striking. This does not challenge Shapin and Shaffer's argument regarding the air-pump, but it does challenge the generalization of the conclusions from their study of experimentation with the air-pump to other areas of experimentation in the Royal Society.

Although there do not appear to have been the same issues as the construction of "matters of fact" through experimentation with the air-pump as described by Shapin and Schaffer, there are several other important issues involving experimentation. Experimentation required a higher level of attention than mere allusions to experiences, which had been reported by investigations of collision by Descartes and Beeckman. Everyone who studied collision wrestled with the complex relationships between "reason" and "experience." However, in addition to these complex relationships, and unlike common experience, the experiments in the Royal Society were deliberate, carefully planned, and were made possible by the "inventive application" of the properties of previously investigated objects, namely, the pendulum.

1.2.1 – Pendulum

Precise experimentation on collision was made possible by the pendulum. A weight at the end of a string was by no means a new invention in the 17th century.[708] What was new, and influential, was the explosion of interest in the properties of this simple object, sparked off in large part by Galileo,[709] beginning in 1602 with his well known letter to Guidobaldo del Monte and continuing throughout his *Dialogo* (1632) and *Discorsi* (1638).[710] Through Galileo the pendulum became linked to discussions of isochrony and timekeeping,[711] as well as the much-debated relationship between weight and the motion of a descending body (or rather the lack of relationship).[712] He had also argued that the circular arc section swept out by a descending pendulum was the brachistochrone—the curve by which a descending body covers the greatest distance in the least time.[713] Building on Galileo's studies, Huygens and the other members of the

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708 Jochen Büttner, "The Pendulum as a Challenging Object in Early-Modern Mechanics," in *Mechanics and Natural Philosophy before the Scientific Revolution*, ed. by Walter Roy Laird and Sophie Roux (Dordrecht: Springer, 2008), 224-6.

709 Galileo's theoretical interest in the pendulum may have been a response to prior practical experiences with the technology. See Büttner, "The Pendulum as a Challenging Object," 226-9.

710 Galileo Galilei, *Opere*, vol. 10 (Florence: G. Barbèra, 1900), 97-100. Galileo Galilei to Guidobaldo del Monte, 29 November 1602. See Piero E. Ariotti, "Aspects of the Conception and Development of the Pendulum in the 17th Century," *Archive for History of Exact Sciences* 8 (1972): 330-2. For a detailed discussion and translation of the letter see Jürgen Renn, Peter Damerow, and Simone Rieger with an appendix by Domenico Giulini, "Hunting the White Elephant: When and How Did Galileo Discover the Law of Fall?" *Science in Context* 14 (2001): 85-9, 133-5.

711 *Opera di Galileo* 7: 475-6. "...the same pendulum makes its oscillations with the same frequency, or very little different—almost imperceptibly—whether these are made through large arcs or very small ones along a given circumference." Stillman Drake, trans., *Dialogue concerning the two chief world systems* (Berkley: University of California Press, 1962) 450. Also see Ariotti, "Aspects of the Conception and Development of the Pendulum," 352.

712 *Opera di Galileo* 8: 139. What has come to be known as the "law of length"—that the period of a pendulum is dependent on the length and not the weight—is presented in the *Discorsi*: "As to the times of vibration of bodies suspended by threads of different lengths, they bear to each other the same proportion as the square roots of the lengths of the thread." Henry Crew and Alfonso de Salvio, trans., *Dialogues Concerning Two New Sciences* (New York: Dover, 1914), 96. Also see Stillman Drake, trans., *Two New Sciences: Including Centers of Gravity & Force of Percussion* (Madison: University of Wisconsin Press, 1974), 97. Also see Ariotti, "Aspects of the Conception and Development of the Pendulum," 352.

713 *Opera di Galileo* 8: 138-9. Galileo's claims regarding the "law of chords" and the brachistochrone are in the *Discorsi*: "...He has clearly shown that the time of descent is the same along all chords, whatever the arcs which subtend them... It is understood, of course, that these arcs all terminate at the lowest point of the

Royal Society used the pendulum as a means to measure the *speed* of a body at impact. Prior to this, the measurements of speed, especially the speeds involved in collision, were difficult to access. For instance, consider the task of measuring the speeds of the balls in a game of billiards. Without some rather sophisticated technology, it is unfeasible to provide anything more precise than general comparative statements such as this ball appears to move more quickly or slowly than that ball. And there are a host of complicating factors such as the effect of rolling along a table, and the impracticality of bodies of unequal size impacting directly along their centers of gravity. Using a system of pendulums, the *speed* of the initially moving body at impact can be related to the *height* (or more specifically to the *angle* from the vertical) from which the pendulum bob was allowed to drop. The resulting speed of a body initially at rest can be related to the height to which it swings after being struck by the initially dropped pendulum bob.

circle, where it touches the horizontal plane. If now we consider descent along arcs instead of their chords then, provided these do not exceed 90°, experiment shows that they are all traversed in equal times; but these times are greater for the chord than for the arc, an effect which is all the more remarkable because at first glance one would think just the opposite to be true. For since the terminal points of the two motions are the same and since the straight line included between these two points is the shortest distance between them, it would seem reasonable that motion along this line should be executed in the shortest time, but this is not the case, for the shortest time - and therefore the most rapid motion - is that employed along the arc of which this straight line is the chord." Crew and Salvio, *Two New Sciences*, 95-6.

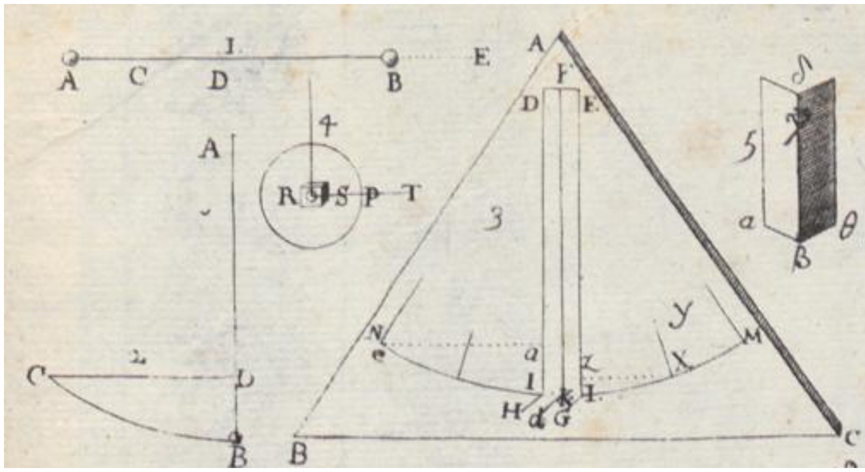

Figure 1. From Edme Marriotte's 1673 *Traitte de la percussion ou chocq des corps dans lequel les principales Regles du mouvement, sont expliquée & demonstrées par leur veritables causes*

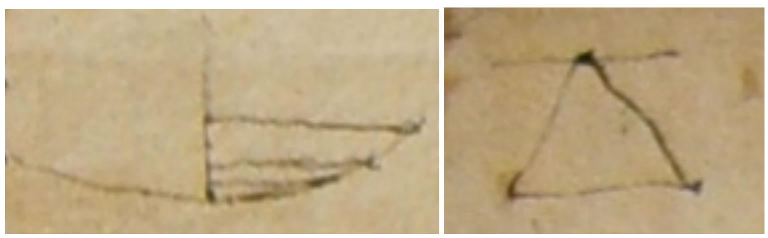

Figure 2. From Christiaan Huygens's 1652 manuscript. *Codices Hugeniorum* 26A, fol. 9r.

In the years after Galileo's work, the pendulum had become an object of much inquiry, debate, and application. It was studied by almost all the major figures in this dissertation, including Beeckman, Descartes, Huygens (most notably), and Wren.[714]

[714] For example, Descartes objected to Galileo's claim of isochrony in letters to Mersenne, arguing that air resistance and various environmental factors would interfere, likely making smaller oscillations slower than larger ones. AT I 73-4, 96. Letters to Mersenne, 8 October, 13 November, and 18 December 1629. Beeckman too had been interested in the effect of air resistance on the pendulum (which he thought would affect small oscillations more than the large), and surmised that a pendulum is only isochronous in a vacuum. Beeckman, *Journal* 1:260. "If you can imagine this [oscillatory motion] as taking place in a vacuum, when only the tendency toward the center of the earth operates, it will perhaps correspond more exactly to what has been said [*i.e.*, perfect isochrony]; for the slowness of the motion [at the extremities of a large oscillation or in small oscillations] is greatly affected by the air with the result that the motion turns out to be even much slower." Quoted and translated in Ariotti, "Aspects of the Conception and Development of the Pendulum," 375. Descartes also claimed that Galileo's "demonstrations" of the circle as the brachistochrone were not sound—they only succeeded in showing that a body descends more quickly

Gradually, several of the factors that complicated the experimental investigation of the pendulum were identified. For example, the period is dependent upon whether or not the line to which the pendulum bob is attached is rigid. If a flexible string is used, the tension is variable through the swing, which affects the amplitude. Also, it matters whether or not the line (regardless of it being flexible or rigid) itself is taken to have weight. Factors such as these, particularly before they were identified, made it difficult to determine which properties should be taken to be essential or nonessential. It was quite possible for an otherwise careful individual, such as Galileo, to believe that a circular pendulum, for example, is isochronous and was shown to be isochronous through experiment, even though the periods of large and small oscillations may or may not be observed to be exactly the same in the given physical pendulum.[715]

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along an arc of a circle than along the chord of the same arc. AT II 379-405. Letter to Mersenne on 11 October 1638. Mersenne too disagreed with Galileo's claim of isochrony, but in the reverse of Descartes: smaller oscillations are faster than large ones. Later, Huygens, corresponding with Mersenne, also argued that the circular pendulum is not isochronous. Like Mersenne he contended that smaller oscillations are faster than large ones, but the periods of large and small oscillations can be made equal if the path of the pendulum is modified. He famously accomplished this by including "cheeks" to restrict the swing, which has the effect of gradually shortening the path of the pendulum bob. This shortened the period for larger amplitudes. Later Huygens, who led the way in the design of workable mechanical pendulum clocks, would show that a pendulum whose bob swept out the path of a cycloid rather than a circle is isochronous.

[715] Paolo Palmieri, "Experimental History," *Endeavour* 33 (2009): 88-93; and Paolo Palmieri, "A phenomenology of Galileo's experiments with pendulums," *The British Journal of the History of Science* 42 (2009): 479-513. The debates, complicating factors, and illusiveness are indicative of an important quality of experimentation, and experiments in the 17th century particularly, which Paolo Palmieri has highlighted: there was no definite boundary between theory and experiment. For example, Palmieri has replicated Galileo's pendulum experiments to show that it would have been quite reasonable for Galileo to believe that his experiments established the independence of the period and amplitude, even though we now know that the pendulum is not isochronous. This marks a distinctly different attitude toward experimentation from that held by Koyré (who alleged that Galileo performed no physical experiments—they were all thought-experiments). Since the pendulum is not actually isochronous, Galileo must have been either incompetent experimentally or never performed experiments. Galileo was not incompetent. So, Galileo's must not have actually performed his experiments on the pendulum. This has also been called the matching problem: "the question of whether Galileo's reports about his experiments really match the outcome of his experiments, whether Galileo's reported outcome or that of our replications." Such a view implies that modern physical theory easily and obviously emerges from experiments. It also assumes that the best way to "understand Galileo's reports [is] from a perspective internal to the texts without considering their meaning in the light of the outcome of the experiments" (Palmieri, "Phenomenology of Galileo," 481). Palmieri, on the other hand, has taken what he calls a "phenomenological stance" and suspends judgment on the meaning of the reports in the texts until he "lives" through the reconstructed experiments. He encounters first-hand the

It should come as no surprise that it was not until Huygens's work and that of the Royal Society that the pendulum was used in the investigation of collision. The properties of the pendulum had gone from a matter of speculation and debate to being described precisely and mathematically by Huygens.[716] And perhaps just as important, the pendulum had captured the imaginations of many in the Society's network as a means of providing universal standards in both time and length.[717] It had come to be an object whose properties were thought to fix other (much sought after) quantities, both practically (the measurement of tempo and pitch in music, the beats of the pulse in medicine, and potentially longitude in navigation) as well as theoretically (*e.g*. universal standard of length). In 1661 Huygens met with Wren, Rooke, Wallis, and other members of the Royal Society, and together they observed experiments involving the collision of two equal-length pendulums. Using a set of algebraic equations, Huygens quickly made a few calculations, and correctly predicted the outcomes of several different trials that the president of the Society, William Brounker, had proposed. No one but Huygens could produce satisfactory rules for accurate prediction, and he scribbled out his equations after

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complexity of experiment. Through his reconstructions, which make it possible to attempt to observe what Galileo would have observed, Palmieri reveals some of the interesting and not so immediately evident difficulties one faces, which blur the lines between theory and experiment. As Palmieri puts it: "In sum, the question is to what extent an experiment has well-defined borderlines that demarcate its confines with theoretical speculation. ... Experience shows things in their dynamic aspect. Theory is a continuation of experiment with cognitive means in order to see the conclusions to which things tend with the mind's eye. An experiment can be cognitively expanded, in other words" (Palmieri, "Experimental History," 89-90). The controversies surrounding Galileo's pendulum point the way for future areas of investigation in which historical studies of collision could be expanded. Namely, future studies would profit from the reconstruction of the Royal Society's experiments with colliding pendulum bobs.

[716] Huygens began to study the pendulum at least as early as age 17, when in 1646 Mersenne posed a challenge to several of his correspondents—determine the center of oscillation of the compound pendulum. Huygens included a proof of this and many other problems in the *Horologium Oscillatorium*, which he started at least by 1659 (but which would not be published until 1673). By 1657 he had invented his first mechanical pendulum clock, and by 1661 he had discovered the tautochrony of the cycloidal pendulum.

[717] Beeckman, Wren, and Huygens had proposed using the pendulum as a standard unit of length. In 1664 the Royal Society carried out experiments on Huygens's universal measure. *Journal* 3:192-3. Sprat, *History*, 314. Birch, *History* 1: 495, 500, 505, 509. Ariotti, "Aspects of the Conception and Development of the Pendulum," 375.

successfully using them.[718] For several months at the end of 1666 more experiments on colliding pendulum bobs were prosecuted at meetings of the Royal Society.[719] Particular attention was given to the hypothesis whether or not motion "dies" or is produced anew. The records show that Wren's documents regarding his experimental work on collision were requested, and a decision was made to continue prosecuting the experiments that he, Croune and Hooke had formerly begun, in order to begin developing "a theory of the laws of motion."[720] In 1667 Sprat's *History of the Royal Society* was published, which singles Wren out and refers to the instrument he used to experiment on collision (and to develop his "doctrine of motion.")[721] The next year the Royal Society returned to prosecuting experiments on collision, with renewed interest in whether or not any motion "dies," and with new interest in investigating the role of hardness and elasticity.[722] At the end of 1668 Oldenburg requested the theories of motion from Wren, Huygens, and Wallis. Later, in 1669 William Neile was asked to present his "Hypothesis of Motion" as well.[723]

Many of the theoretical hopes for the properties of the pendulum proved to be illusive. Galileo thought he had experimentally shown that his pendulum is isochronous, but we now know that it is not. Similarly, he did not successfully demonstrate that the circular arc is the brachistochrone. Moreover, due to gravitational variations, the pendulum cannot be used to provide a universal standard of length, as Beeckman and

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[718] HOC 5: 547. Oldenburg to Spinoza, 18 December 1665. Also see OCH 2: 624-5. Moray to Oldenburg, 27 November 1665.
[719] Birch, *History* 2: 116-7 (17 October 1666). Ibid., 117 (24 October 24 1666). Ibid., 131-2 (4 December 1666).
[720] Birch, *History* 2: 140 (16 January 1666/7).
[721] Sprat, *History*, 312.
[722] Birch, *History* 2: 320 (12 November 1668). Also see the extensive correspondence on this topic between Wallis and Neile. See chapter 5.
[723] Ibid., 361-2. See OCH 5: 517-524 for the text of "William Neile's Hypothesis of Motion" and OCH 5: 524-528 for Hall's English translation.

Huygens had hoped.[724] Nevertheless the intellectual excitement regarding the pendulum was key for the study of collision in the Royal Society.

Section 2
Principles

Experiments on collision increased in number and sophistication upon the introduction of the pendulum. In addition to experimental investigation, the rules of collision were formulated with principles. These principles were conceptualized through the interaction with objects such as the pendulum and the balance. Bertoloni Meli has described several patterns of transformation in seventeenth-century mechanics, in which objects were used by mathematicians and natural philosophers, proving to be key in conceptualizing problems.[725] A process described as "dematerialization" was particularly important to the study of collision. It refers to "transformations involving the removal of material constraints through a process of mental abstraction ... the same proportions or relations valid for the constrained case were supposed to remain valid also in the unconstrained one."[726] For example, Huygens's famous variant of the Torricelli

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724 Ariotti, "Aspects of the Conception and Development of the Pendulum," 407-9.
725 Domenico Bertoloni Meli, "Patterns of Transformation in Seventeenth-Century Mechanics." *The Monist* 93 (2010): 578-95. These include "unmasking," "morphing," and "dematerialization." The first involves "the recognition that apparently complex and elaborate objects or devices can be shown to consist of simple, known ones in disguise, as in a metaphorical removal of a veil or a mask. In these cases simple visual inspection—at times with minimalist interventions—enabled the reduction of several seemingly intractable cases to established ones. The term "unmasking" captures the minimal intervention required in these cases." "Morphing" on the other hand, "required some degree of intervention and elaboration: the issue was not simply to point to a different way of looking at an object by metaphorically removing a veil or a mask, but to perform a series of operations—in line with my characterization of thinking with objects, either mentally or experimentally, with thought and real experiments—leading from one object or device to another. [...] the term 'morphing' [...] capture[s] these creative transformations."
726 Bertoloni Meli, "Patterns," 579, 588-9. As examples, Bertoloni Meli cites the transformation from the constrained case of the (pseudo-Aristotelian *Quaestiones mechanicae* account of the) balance in equilibrium to the unconstrained case of "moments of speeds" by Galileo; the transformation of the balance

principle—which he likely used in connection with his principle of the reversibility of impact and Galileo's law of fall, to determine the conservation of body times speed squared[727]—was formulated by a transformation involving the removal of material constraints. The original principle states, "two connected heavy bodies cannot move by themselves unless their common center of gravity descends." Huygens's key modification is to "disconnect" the bodies and to consider them in motion, but maintains the relation between the bodies. He claims that if the line along which the colliding bodies move is converted from the horizontal to the vertical, then the center of gravity of the system of disconnected, but colliding, bodies cannot rise after impact. It is no coincidence that in colliding pendulums horizontal motion is converted to vertical motion.[728]

Similarly, the relations of quantities of a balance in equilibrium—speed, weight, and inverse proportion—were "dematerialized" in Beeckman's *Journal*, Descartes's early and later accounts of collision, as well as the theories of Huygens, Wallis, and Wren. Although many drew from the same source, the abstracted relations of quantities in a balance were used to conceptualize different facets of collision. The speeds and bodies before and after impact are "balanced" (the dematerialization of the pseudo-Aristotelian balance). And a particular scenario of collision—when the respective speeds of each body are inversely proportional to the sizes—is recognized as having a privileged status, namely the speeds before and after impact are the same. The notion of equilibrium has an important relationship with other fundamental concepts in the theories of collision, such

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to the unconstrained case of collision by Beeckman, as well as the visual representation of collision by both Wren and Huygens. He also cites the transformation of the Torricelli principle to the unconstrained case of Huygens's variant of the principle.

[727] See chapter 4, section 3.

[728] HOC 16: 21-5, 95n. Bertoloni Meli, "Patterns," 589. Bertoloni Meli, *Thinking with Objects*, 233. Christiane Vilain, "Christiaan Huygens' Galilean Mechanics," in *The Reception of the Galilean Science of Motion in Seventeenth-century Europe,* ed. by Carla Rita Palmerino and Thijssen (Boston: Kluwer Academic Publishers, 2004), 195.

as the principles of conservation, the contest view of collision, as well as principles of symmetry and relative motion.

2.1 – Inverse proportion of bodies and speeds

Isaac Beeckman used the relations of quantities in a balance from the pseudo-Aristotelian *Mechanical Problems* to conceptualize collision. Recall that, according to this tradition, the equilibrium of the balance is explained by the inverse proportion between the weights and speeds of the weights. A heavier weight closer to the fulcrum moves slower than the lighter weight farther from the fulcrum, when the balance rotates about the fulcrum in a circle. Operating under the presumption of equilibrium, the initially moving body is the "lighter weight." After collision the bodies move together as an aggregate, which is due to Beeckman's understanding of the hardness of atoms. The aggregate is the "heavier weight." Since the speeds of the lighter weight and heavier weight are inversely proportional to the weights, the speed of the aggregate can be determined. The weights and motions of the balance are abstracted to include the body and aggregate body before and after collision. Even in those scenarios in which the bodies are both initially in motion, Beeckman first (attempted) to transform the scenario into one in which one of the bodies is initially at rest. Beeckman employed multiple strategies to accomplish such transformations.

Although Descartes's notions of both bodies and "conservation" are different from Beeckman's (Beeckman was interested in atoms, Descartes was not; Beeckman was interested in the destruction of motion, Descartes asserted that the "quantity of motion" remains always the same in the world), Descartes's early work on collision appears to be conceptualized according to the pseudo-Aristotelian balance in a remarkably similar manner.[729] In Descartes's later view of collision, in which he stipulates both the transfer of motion and rebound, he retains this framework for those scenarios of collision in which the force of resistance is overcome and motion is transferred upon impact (*i.e.* rules 5, 3, and 7a).

As discussed in chapter 5, Wallis's "Foundation for all Machines for Facilitating Motion" was likely developed from the lever:[730]

> For in whatever ratio the weight is increased, the speed is diminished in the same ratio, whence it is that the product of the weight and the speed for any moving force is the same.[731]

On this foundation Wallis built his theory of collision. Being based on the proportions of "all machines," specifically the lever, which is designed to cause weights to be moved, his rules of collision focuses on the forces that cause motion. This is one of many significant differences between his theory of collision and those of his contemporaries and predecessors. Nevertheless, in part because the bodies, which are the focus of his theory, do not rebound after collision, his manner of determining the consequences of various collisions is nearly identical to those of Beeckman and the early Descartes (even though Wallis used a dramatically different form of mathematics). Like Beeckman and Descartes's early work, the inverse proportion is among the body in motion before collision and the aggregate in motion after collision.

In contrast to Beeckman and Descartes, Wren used the same inverse proportion of the sizes of bodies to their speeds, drawn from a balance in equilibrium, but he employed

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[729] See chapter 3, section 4.1.
[730] See chapter 5, section 3.1.1.
[731] John Wallis, "Summary Account of the General Laws of Motion," *Philosophical Transactions* 3 (1669): 864-5. Translation by Hall, OCH 5: 168.

it to distinguish between what he called "proper" and "improper" velocity. Rather than describe the speed of a body before impact and the speed of the aggregate after impact, "proper" velocity refers to those scenarios in which the initial velocities of bodies are inversely proportional to the sizes of the bodies themselves. "Improper" velocities are just those velocities that are not inversely proportional to the sizes of the bodies. Wren's law of nature states that bodies having proper velocities retain them after collision, although they move in a contrary direction. As we have seen in chapter 5, the equivalence between collision and the balance is explicit for Wren. Even in his extremely spare prose, he expends a sentence to make the connection: "For this reason the collision of bodies having their proper velocities is equivalent to a balance swinging about its center of gravity," just as "the collision of bodies which have improper velocities is equivalent to a balance reciprocating upon two centers equidistant either side of the center of gravity: for the balance may be extended into a yoke when the need arises."[732]

Huygens had considered using the relation described by Wren's proper velocity (and subsequent first half of Wren's law of nature) as an axiom in his theory of collision. Ultimately Huygens decided against it, in favor of a proposition that he considered to be even more apparent. But the balance remained important for Huygens, particularly the notion of center of gravity.[733] The importance of the balance for Wren and Huygens can be seen not just in the principles of their theories, but also in the manner in which they visually represented collision in their texts.

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[732] Christopher Wren, "Lex naturae de collisione corporum," *Philosophical Transactions* 3 (1669): 867-8. It has also been collected in OCH 5: 319-20, with an English translation by Hall, OCH 5: 320-1.

[733] Christiaan Huygens, "Regles du mouuement dans la rencontre des corps," *Journal des sçavans* 2 (1667-71): 531-6. It is also collected in HOC 16: 179-81 (also see HOC 6: 383-5, Huygens to Gallois, 18 March 1669). For example, the *Regles* culminates in an 8th (unnumbered) proposition which states that, "the common center of gravity of two or three (or such as one wishes) bodies, always advances equally toward the same side in a straight line before and after impact." Translation by Iltis, "Controversy over Living Force," 50.

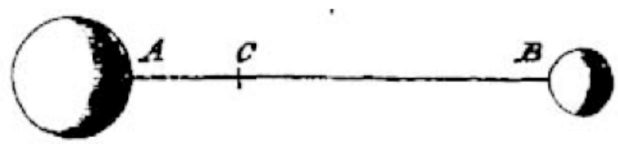

Figure 3. De motu corporum ex percussione

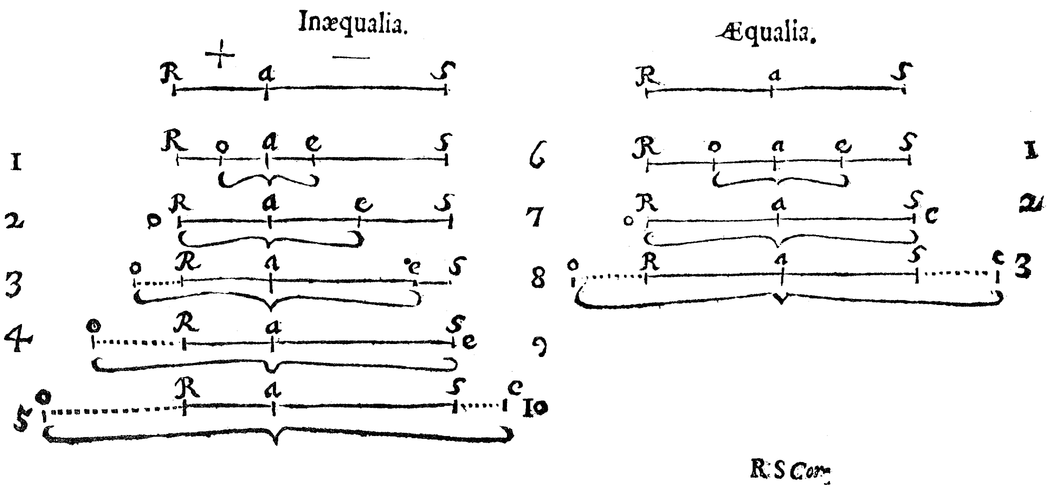

Figure 4. Lex naturae de collisione corporum

2.2 – Equilibrium and conservation

Equilibrium has a complicated relationship with notions of conservation. Some historians have pointed to Beeckman's work as a source of the conservation of quantity of motion.[734] Since Beeckman uses the inverse proportion of speeds and bodies before and after collision, and since this proportion describes a balance in equilibrium, the presumption has been that he must have assumed that the product of speed and body remains the same before and after collision. However, nowhere does Beeckman assert such a claim. Rather, as I have shown in chapter 2, Beeckman was particularly concerned, *not* with conservation, but rather with what he called the destruction of

[734] Richard Arthur, "Beeckman, Descartes and the Force of Motion," *Journal of the History of Philosophy* 45 (2007): 3.

motion. Each of his mathematical studies of collision throughout his *Journal* is in the context of his interest in the loss of motion with each impact.[735] Beeckman uses the word "conserve" in his *Journal*, but it is in his description of God continually counteracting what Beeckman's mathematics show. For Beeckman, God must be continually enlivening and conserving motion, otherwise, motion in the world would continually (and fairly rapidly) decrease and cease. A similar issue arises with Wallis, who uses a similar inverse proportion to conceptualize collision. He too is explicit in his rejection of the conservation of motion. When asked the question directly by William Neile, "Whether no Motion in the World perish," he responded that motion may be extinguished and become equivalent with rest.[736] However, an important distinction should be made between the historical actors explicit interests and the historians' retrospective analysis. A "conserved quantity" may be implicit in the theories of Beeckman and Wallis, but their explicit position was that the quantity that was of interest to them was *not* preserved in collisions.

The question whether or not motion remained constant or is destroyed was debated with enthusiasm among members of the Royal Society as evidenced, for example, in the lively correspondence between William Neile and John Wallis (the latter arguing that motion can be destroyed),[737] as well as Willughby's and Croone's criticisms

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[735] Beeckman, *Journal* 1:265-7; 2:45-7; 3:128-9; 3:363-4.

[736] OCH 5: 274. Wallis to Oldenburg, 21 December 1668. "*Whether no Motion in the World perish, &c;* that is, (as you now explain it,) *whether any of that motion, that was first* (or at any time since) *impressed in matter be lost, or* (onely) *communicated from one parcell of the matter to another; so that though this or that body do cease to be moved, that the motion itself ceaseth or perisheth not.* [...] [It] is a question, which I find Mathematicians, as well as Naturalists, sparing to determine positively; and, you know, I am sparing and wary in asserting Universall Negatives. Yet you have, to this, my answer, full inough, (if it be observed,) in my answere to ye fourth. For I there intimate my judgement, that *motion may be extinguished*, & I shew you *how*; that is, a Motion compounded of two contrary forces, may be extinguished by each other, & become equivalent with Rest."

[737] OCH 5: 263-5 (Neile to Oldenburg, 18 December 1668), 272-5 (Wallis to Oldenburg, 21 December 1668), 286-7 (Neile to Oldenburg, 28 December 1668), 302-4 (Wallis to Oldenburg, 2 January 1668/9), 312-4 (Neile to Oldenburg, 2 January 1668/9), 336-8 (Wallis to Oldenburg, 12 January 1668/9), 346-7 (Neile to Oldenburg, 22 January 1668/9), 363-4 (Neile to Oldenburg late January 1668/9), 517-8 (Neile to

of both Wren's and Huygens's published theories of motion in the *Philosophical Transactions*.[738] Huygens, for instance, consistently wrote that when two equal bodies meet moving at the same speed, they rebound with the *same speed* with which they approached. He does not write that they rebound with speeds equal in magnitude to their original speeds. He writes that it is the same speed. Impact is instantaneous. Motion is not interrupted. The direction merely changes. And, according to Huygens, a change of direction did not mean a change in speed.[739] The topic becomes complex when collision is understood algebraically rather than geometrically.

There was widespread disagreement on not only the answers given to the question of whether motion remains constant, but there was disagreement on what phenomenon or "physical quantity" was even under investigation with the question. Isaac Beeckman had been compelled by his mathematical investigations of collision to conclude that *motion* itself must in fact decrease and die, but God—operating contrary to what Beeckman's mathematical argument showed—continually "enlivens" motion. Whether or not Descartes was influenced by his colleague on this topic, Descartes maintains the connection between God and motion. However, rather than a God who must continually intercede to revive and conserve motion, Descartes's God is immutable (as a consequence of Descartes's God's perfection). Therefore, God, always acting the same way, maintains the same *quantity of motion*. In other words, Descartes set as a fundamental principle that

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Oldenburg, 7 May 1669), 540-2 (Wallis to Oldenburg 10 May 1669), 550-1 (Wallis to Oldenburg, 17 May 1669), 558-9 (Neile to Oldenburg, 20 May 1669), 573-4 (Wallis to Oldenburg, 29 May 1669).

[738] A. Rupert Hall, "Mechanics and the Royal Society, 1668-70," *The British Journal for the History of Science* 3 (1966): 35-37. Hall provides an overview of the concerns of Croone, Willughby, and Neile. Also see Dana Jalobeanu, "The Cartesians of the Royal Society," in *Vanishing Matter and the Laws of Motion: Descartes and Beyond*, eds. Dana Jalobeanu and Peter R. Anstey (New York: Routledge, 2011), 103-129, as well as J. F. Scott, *The Mathematical Work of John Wallis, D.D., F.R.S. (1616-1703)* (London: Taylor and Francis, LTD., 1938), 104-6.

[739] Richard Westfall, *Force in Newton's Physics: The Science of Dynamics in the Seventeenth Century* (New York: American Elsevier, 1971), 156.

the *quantity of motion* (which was proportional to both body and motion) is conserved. The impact law describes how the same *quantity of motion* is maintained when bodies interact, and the rules of collision provide further specification for the interaction of bodies of different sizes and speeds. Contrary to Descartes, Huygens argued that the *Cartesian quantity of motion* can increase or decrease, and, relying on the notion of the relativity of motion (which Descartes would not have accepted in this form), he persuasively demonstrated this.[740] This appears to have been the quantity that concerned Willughby. He seems to have thought that the notion that it could be destroyed or "created from nothing" defied common sense—it was "absurd" and "incredible."[741] However, as Huygens would also claim, if direction were considered (*i.e*. if motion in a contrary direction is *subtracted*), then the "quantity of motion" would be conserved.[742] Although not immediately grasped by his contemporaries, the universality of this principle must have been astounding. Regardless of other complicating factors such as the kind of body, whether it is hard, soft, or elastic, this principle remains valid. Adding more complexity to the topic, Huygens also introduced a new principle—the conservation of the product of body and the square of its speed (later known as the *vis viva*), however this principle was only valid for perfectly hard bodies (*i.e.* perfectly elastic bodies).

[740] HOC 16: 49. In Proposition 6 of *De motu corporum ex percussione* Huygens demonstrates the following: "When two bodies collide with one another, the same quantity of motion in both taken together does not always remain after impulse what it was before, but can be either increased or decreased." Translation by Mahoney.
[741] Hall, "Mechanics and the Royal Society," 35.
[742] HOC 6: 429-33. Huygens, "Regulae de Motu Corporum ex mutuo impulsu," 925-8. Huygens includes this claim of the conservation of quantity of motion with direction in the paper published with the *Philosophical* Transactions, which also states that the Cartesian quantity of motion can increase, decrease, or remain the same. *Quantitas motus duorum Corporum augeri minuive potest per eorum occursum; at semper ibi remanet eadem quantitas versus eandem partem, ablata inde quantitate motus contrarii.* "The quantity of motion which two hard bodies have may be increased or diminished by their collision, but when the quantity of motion in the opposite direction has been subtracted there remains always the same quantity in the same direction." Translation by Hall, "Mechanics and the Royal Society, " 34.

2.3 – Equilibrium and the "contest view"

Several of the theories of collision employ the notion of a contest between opposing forces. Descartes's impact law describes a contest between the moving force and the force of resistance. Marcus Marci described collision in terms of a contest between *impulse* and resistance, and John Wallis did so in terms of *impetus* and *impedimentum*. This particular manner of conceptualizing collision as a contest between forces did not arise from the balance. Rather, it likely stems from commentaries on Aristotle's *Physics* (book VII) and *De caelo*, in which the relationships between motive force, motion, and the force of resistance are discussed. Because of its central role in Descartes's impact law and rules of collision, the contest view has an important place in the development of the theory of collision. In the context of collision, it bears a relationship with the concepts of equilibrium, and in Descartes's rules of collision it combines to produce a theory that is strikingly different from most of his peers and successors.

Throughout Descartes's early and later views of collision, impact is governed by a contest between the force of motion and the force of resistance. If the force of resistance is greater than the force of motion, impact results in rebound. If the force of motion is greater than the force of resistance, impact results in a transfer of motion between the bodies. Unlike other common notions such as the unity of opposites,[743] this contest is not in equilibrium. The outcome depends on whether one or the other of the forces is greater.

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743 In the opening pages of the pseudo-Aristotelian *Mechanical Problems*, the author directly connects mechanical movement to the lever, the lever to the balance, the balance to the circle, and the circle to the "marvelous" unity of opposites: "Everything about the balance is resolved in the circle; everything about the lever is resolved in the balance, and practically everything about mechanical movement is resolved in the lever." [pseudo] Aristotle, *Mechanical Problems*, in *Minor Works*, trans. by Walter Stanley Hett (Loeb Classical Library, Cambridge, Harvard University Press, 1936), 334. Translation by Winter, "Mechanical Problems," 2. According to the author, the marvel of mechanical advantage stems from something even

In his early writings on collision, Descartes focuses solely on cases in which the force of motion is greater than the force of resistance, and motion is transferred. His early examples of the transfer of motion are the same as those of his older colleague Beeckman, who described collision *only* in terms of the transfer of motion, and did *not* advocate a contest view. Unlike his colleague, Descartes indicates that it is possible that the force of motion may not overcome the force of resistance in some situations, but in his early view he does not stipulate when the force of resistance is greater than the moving force. The outcome (*i.e.* the transfer of motion) of the contests that Descartes does present (many of which include smaller bodies transferring motion to larger bodies at rest) is governed by the principle of equilibrium that was described in the previous section.

In Descartes's later view on collision, specifically in the rules of collision in the *Principles of Philosophy*, Descartes stipulates when the force of resistance is greater than the moving force. This is at the center of the change in Descartes's view of collision. According to rule 4, the force of resistance of a larger body at rest cannot be overcome by

more marvelous—the unity of opposites in the circle—which, according to the text, is as it should be. "The circle contains the first principle of all such matters. This falls out quite logically: it is nothing absurd for a marvel to stem from something more marvelous still, and most remarkable is for there to be opposites inherent in each other, and the circle is made of opposites" (ibid.). The opposites to which the author refers include the following: the circle "derives from the moving and the standing, whose nature is opposite each the other;" "the perimeter...generates opposites: the hollow and the curved;" and "it moves backwards and forwards at the same time" (ibid.). While discussing equilibrium in the context of the *Mechanical Problems* and Bernardino Baldi's translation, commentary, and criticism of the text, Beeckman argued, contrary to Baldi, that a balance in equilibrium could be understood in relation to motions: "The cause of equilibrium therefore can be motion, even if the bodies in equilibrium are not moved. For the cause of equilibrium is past and future motion. During the present, to be sure, the body is at rest because past and future motions occasion rest." Beeckman, *Journal* 3:134. Translation quoted in Schuster, "Descartes and the Scientific Revolution (dissertation)," 68. According to Paolo Palmieri, "Bernardino Baldi debunked the miraculous nature the circle, trying, as he saw it, to correct the errors of the pseudo-Aristotle by means of Archimedes and Guido Ubaldo." See Paolo Palmieri, "Breaking the circle: the emergence of Archimedean mechanics in the late Renaissance" *Archive for History of Exact Sciences* 62 (2008): 306-7, 325-9. In the context of collision, Beeckman claimed that when both bodies are initially in motion with speeds that are inversely proportional to the sizes of the bodies, after they meet, the bodies will come to rest: "*Sic etiam ratiocinandum de bilance*." Beeckman, *Journal* 3:133. Also see Bertoloni Meli, "Patterns," 589.

the moving force of a smaller body. In this scenario, and those described by rules 2 and 7b, which as I have argued are extensions of this rule, no motion is transferred and the smaller body rebounds retaining all of its motion.[744] Both the transfer of motion and rebound are governed by Descartes's prior principle of the conservation of "quantity of motion."

Descartes's rules of collision also include scenarios, which he had not expressed in his early view, in which there is no obvious "winner" in the contest between the force of resistance and the moving force. The contest is balanced. Since there is no way to decide between rebound or transfer in rules 6 and 7c, for example, Descartes splits the difference. He determines how much motion would be transferred or retained, if either outcome occurred, and divides that amount between the two options.[745] The outcomes that he describes for these scenarios are idiosyncratic. For instance, upon the impact of equal bodies, in which one body is at rest, as described by rule 6, the initially moving body transfers part of its motion and retains part of its motion. This is due to the balance between forces. And this is unlike most other accounts of collision which claim that either both bodies move together after impact with half the speed of the initially moving body (which was also Descartes's early view), or the initially moving body stops and the body initially at rest moves with the speed of the initially moving body. Descartes uses the same reasoning for rule 1, in which equal bodies collide with equal speeds. However,

[744] The received view claims that the force of resistance in a body at rest is equal to the product of the size of the body and the speed of the body approaching it. As I have argued in chapter 3, this is incorrect. It may have seemed plausible those who proposed it, due to an algebraic reconstruction of Descartes's rules of collision, but this places too little attention on the particularities of the mathematics in which Descartes conceptualized collision—a topic to which we will return below.

[745] See chapter 5, section 5.4.3

a transfer of motion in this scenario would conflict with his prior principle of the conservation of quantity of motion. So, both bodies rebound retaining all of their motion.

2.4 – Symmetry: equilibrium and relativity

The concept of symmetry was an important notion in the conceptualization of collision. The symmetries from the balance determined epistemically privileged scenarios of collision, and the notion of a quantity remaining the same through some change was fundamental, particularly to Huygens's theory of collision. There have been multiple meanings of the word symmetry, such as the aesthetic Vitruvian notion of an agreeable correspondence of parts to the whole, or the abstract mathematical notion of invariance in a transformation. In what follows two poles of a disagreement regarding the historical emergence of the mathematical concept of symmetry are distinguished, and I clarify the position I have taken in this dissertation. We then review the fundamental importance of symmetry as a property as well as a relation in the theories of Wren and Huygens.

2.4.1 – Historical emergence of symmetry vs. Universality of symmetry

No one in the seventeenth century, featured in this dissertation, used any form of the word "symmetry" in his theory of collision. Nevertheless, I have used this word to describe aspects of the theories. Some historians, such as Bernard Goldstein and Giora Hon have forcefully argued[746] that what they call "scientific symmetry" (defined as "a relation [...] which under certain classes of transformations, such as rotation, reflection,

[746] Giora Hon and Bernard R. Goldstein, *From Summetria to Symmetry: The making of a revolutionary scientific concept* (New York: Springer, 2008).

inversion, or other abstract operations, leaves something unchanged—invariant"[747]) did not begin to emerge until 1794 with Legendre's "revolutionary definition" of symmetry in his *Élements de géometrie*.[748] They argue that all instances of the term up to 1794 refer to a radically different set of concepts. Prior to 1794 "symmetry" primarily referred to a *property* or a *single entity*. In an aesthetic evaluative context, for example, it referred to an entity being "well proportioned."[749] Legendre's definition, on the other hand, "severed" the term from its "traditional roots and endowed it with a novel meaning, equality by symmetry of two nonsuperposable solids—a relation which is based on inverse ordering."[750] After 1794, symmetry began to refer to a *relation* between entities. Hon and Goldstein argue that the revolution in the concept of symmetry shows that, contrary to the presumptions of notable thinkers such as Eugene Wigner, Hermann Weyl, and George Sarton (and many other historians),[751] symmetry "is not an innate concept that has been with us, so to speak, from the dawn of humanity."[752] Hon and Goldstein's argument implies that it would be entirely inappropriate to use the term "symmetry" to describe aspects of the work of Huygens and Wren, since the concept in question did not yet exist.

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[747] Hon & Goldstein, *Summetria to Symmetry*, 2.
[748] The definition in Book V, Proposition 23: "Two equal solid angles which are formed (by the same plane angles) but in the inverse order will be called angles equal by symmetry, or simply symmetrical angles." Quoted in Hon & Goldstein, *Summetria to Symmetry*, 2. According to Hon and Goldstein this "marks the watershed in the history of the scientific concept of symmetry."
[749] Hon & Goldstein, *Summetria to Symmetry*, 2.
[750] Ibid., 49.
[751] According to Wigner: "Symmetry and invariance considerations, and even conservation laws, undoubtedly played an important role in the thinking of physicists, such as Galileo and Newton, and probably even before them. However, these considerations were not thought to be particularly important and were articulated only rarely." Quoted in Hon & Goldstein, *Summetria to Symmetry*, 3. And Sarton had challenged other historians: "It would be fascinating to retrace the development of the idea of symmetry from the Pythagorean days down to our time. Such a study would enable us to make a master section through the whole history of scientific thought and would provide us with an excellent touchstone to appreciate the relations of science and art at various times. This examination would be very comprehensive, for it would take us into almost every department of knowledge; it would attract us into the workshops of the craftsmen as well as into the laboratories of the scientists; it would oblige us even to make a pleasant excursion in the realm of Chinese philosophy and aesthetics. Professor Jaeger himself might be tempted to carry on these investigations." Quoted in ibid.
[752] Hon & Goldstein, *Summetria to Symmetry*, 49.

However, the methodology of Hon and Goldstein's extensive work suffers from a flaw. To defend their argument, they track, with great care, many instances of the term "symmetry" throughout the historical record. In each case they show that the term in its historical context did not refer to the modern abstract concept of symmetry (or even the less abstract 18th century notion of "bilateral symmetry"). They then conclude that the modern concept of symmetry did not exist prior to 1794. This is the wrong conclusion. Their work provides evidence for the conclusion that the meaning of the term "symmetry" changed. It does not show that the concept now associated with the term symmetry came into existence at the end of the 18th century. It is possible that the concept—a transformation in which something remains unchanged—predates its association with the term "symmetry." It may have existed under a different name, or under no articulated name at all.

Physicist György Darvas is on the other side of the spectrum from Hon and Goldstein regarding symmetry.[753] Darvas, inspired by Hermann Weyl's lectures on symmetry, refers to it as both a *phenomenon* deeply fundamental to nature and a *concept* widely shared by people throughout diverse cultures. He sees symmetry as a bridge connecting the arts and the sciences, and implicitly the past and present. According to Darvas, "one of [his] book's goals is to present the unity and interdisciplinary nature of human culture,"[754] and symmetry is the key. Unlike Hon and Goldstein, who focus their attention on the historical meaning of a term (and perhaps a concept), Darvas makes a

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[753] György Darvas, *Symmetry: Cultural-historical and ontological aspects of science-arts relations; the natural and man-made world in an interdisciplinary approach*, trans. David Robert Evans (Boston: Birkhäuser, 2007). Darvas has been the director of the *Symmetrion* (an institute dedicated to the interdisciplinary study of symmetry, which publishes the journal *Symmetry: Culture and Science*) since 1991, and is the Honorary CEO of the "International Symmetry Association," which formed in 2003.
[754] Darvas, *Symmetry*, x.

tripartite distinction in the notion of symmetry: a phenomenon, a concept, and an operation. "The *phenomenon* is what we consider to be symmetrical on the basis of our experience or of knowledge we have learned. The *concept* is what circumscribes all such phenomena. The *operation* is what gives rise to the phenomenon or makes it possible."[755]

Unlike Hon and Goldstein who restrict their attention to the historical actors' categories, Darvas presumes that symmetry is timeless. Unlike Hon and Goldstein who trace instances of the *word*, Darvas documents instances of *a very general notion* of symmetry: "in the course of any kind of [...] transformation (operation) at least one [...] characteristic of the affected (arbitrary and not necessarily geometrical) object remains invariant (unchanged)."[756] He claims that this notion can be found in physics, the earth sciences, astronomy, crystallography, chemistry, biology, psychology, music,[757] literature,[758] dance, fine art,[759] applied art, architecture, ethics,[760] logic and philosophy, and economics,[761] to name a few. Aspects of Huygens's and Wren's theories would certainly be accepted in this broad notion of symmetry as *sameness amid change.*[762]

I would like to chart a middle path between Hon and Goldstein, and Darvas. Huygens and Wren would not have understood aspects of their work as instances of the

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755 Ibid., 1.
756 Ibid., 20.
757 Ibid., 34. Examples include "the rules of rhythm, melody, and the architecture of the musical work."
758 Ibid. Examples include "rhyme, meter, the structure of the literary work."
759 Ibid. Examples include "proportions, perspective, and harmony of proportion and colour."
760 Ibid. Examples include "virtuous behaviour, finding moderation, the middle way."
761 Ibid. Examples include "balance, asymmetric decision-making in game theory, simulation."
762 Ibid., 56, 7. Darvas notes as an example in his historical survey that the seventeenth century "was when Galilei expounded his relativity principle in which he proclaimed the equivalence of the various inertia systems." Darvas also claims that there is a similarity between "natural laws" and symmetry in the following way: "the road to their discovery presuppose[s] the repeatability and reproducibility of the experiments (events), irrespective of their time and place. Furthermore, they declare the natural laws thus found and stated to be independent of time and place. If we recall the generalized definition of the concept of symmetry, do we not see a similarity?" The same experiment conducted in different times and places (ideally) produces the same results. "To this day it is this symmetry that is the highest criterion of scientific endeavour."

concept referred to by 17th century usages of the term "symmetry." The word at the time would have referred to Vitruvius's "aesthetic" meaning of the term, "a due proportion of each part in respect of the whole," or possibly Euclid's "mathematical" meaning of the term, "commensurability," but not the modern notion of invariance in a transformation.[763] Nevertheless, there are identifiable patterns of thought throughout history and particularly among Huygens and Wren, which include recognition of those things that remain the same throughout changes—and the application of this recognition in their theories of motion and collision. While I recognize that the historical actors did not use the term "symmetry," and that the publication of Wren's and Huygens's theories of collision did not contribute to changes in the meaning of the term "symmetry," throughout chapter 5 I have referred to aspects of their theories using the term symmetry as a short hand for the general notions of transformations in which a characteristic of the affected object remains unchanged. The concept of symmetry, in this sense, played an important role as a non-empirical principle in Huygens's and Wren's theories of collision.

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[763] The first edition of Thomas Blount's *Glossographia, or A dictionary, interpreting all such hard words, whether Hebrew, Greek, Latin, Italian, Spanish, French, Teutonick, Belgick, British, or Saxon, as are now used in our refined English tongue* (1656) defines the word as follows: "Symmetry (*symmetria*) due proportion of each thing to other, in respect of the whole. The convenience that runs between the parts and the whole." Edward Phillips's *The New world of English words, or A general dictionary containing the interpretations of such hard words as are derived from other languages* (1658) defines the word in a similar manner: "Symmetry, (Greek) a due proportion of each part in respect of the whole." In the 7th edition of Phillips's dictionary (1720), the definition is much the same, but expanded to include the arts and medicine: "Symmetry, (in *Architecture,* Painting, &c.) Uniformity, a due Proportion requisite according to the respective Rules of those Arts, to make all the Parts of the Work to agree to and with the Whole: Among Physicians, it is sometimes taken for a good Temper of body." Hon and Goldstein provide a translation of Vitruvius's definition of symmetry: "the appropriate agreement of the elements of the work itself, a correspondence [*responsus*], in any given part, of the separate parts to the entire figure as a whole. Just as in the human body there is a symmetric quality of eurhythmies [*symmetros est eurythmiae qualitas*] expressed in terms of the cubit, foot, palm, digit, and other small units, so it is in perfect works [of architecture]." Quoted in Hon & Goldstein, *Summetria to Symmetry*, 5. Regarding Euclid's *Elements*, book 10, definition 1: σύμμετρα μεγέθη λέγεται τὰ τῷ αὐτῷ μέτρῳ μετρούμενα, ἀσύμμετρα δέ, ὧν μηδὲν ἐνδέχεται κοινὸν μέτρον γενέσθαι. "Those magnitudes are said to be commensurable which are measured by the same measure, and those incommensurable which cannot have any common measure." Translation by T. L. Heath, *The thirteen books of Euclid's Elements*, vol. 3 (Cambridge: Cambridge University Press, 1908), 10.

2.4.2 – Symmetrical collisions

We have referred to symmetry as a property of an entity (a whole whose parts are in agreeable proportion) and as a relation (a transformation in which something remains invariant). The former notion (particularly if bilateral symmetry is included as an agreeable proportion among parts), is connected to the balance, and thus to several of the theories of collision. For instance, it is apparent in Archimedes' demonstration of the "law of the lever" in book I of *On the Equilibrium of Planes*, which states that two commensurable magnitudes balance at distances inversely proportional to the magnitudes. The book contains seven postulates and fifteen propositions; the law of the lever is the sixth of the propositions.[764] His demonstration relies on the previously established 4th and 5th propositions, and ultimately on an assumption, *i.e*. the first postulate, which states, "equal weights at equal distances are in equilibrium, and equal weights at unequal distances are not in equilibrium but incline towards the weight which is at the greater distance." Although Archimedes would not have referred to it as such, the first postulate is a statement of symmetry.[765]

Often represented diagrammatically as a balance of equal weights at equal distances, the "symmetrical case of collision" (equal bodies with equal speeds moving toward each other) is given a position of importance in several of the theories of collision.

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[764] T. L. Heath, *The Works of Archimedes* (Cambridge: Cambridge University Press, 1897), 192.

[765] Commentators do not agree on the status of equilibrium in Archimedes's works. Compare the following two passages from Palmieri and Roche. According to Palmieri, mechanical theorists noticed that Archimedes did not rely on *a priori* principles to derive equilibrium, but rather that he had *postulated* equilibrium. "In his footsteps, these mechanical theorists embraced the empirical nature of equilibrium, that is, they postulated principles of equilibrium that depend on certain elementary facts of experience. This moves towards empiricism, and away from *a priori* principles, opened up new vistas about the science of mechanics." Palmieri, "Breaking the circle," 306. Roche, on the other hand, claims that Archimedes's first postulate does not appeal to common experience, and that it may well have appealed to symmetry or "indifference." Roche claims that "The Archimedean tradition in physics preferred justification by symmetry, whenever possible, to justification by experiment." John Roche, "A critical study of symmetry in physics from Galileo to Newton," in *Symmetries in physics (1600-1980)*, ed. Manuel Garcìa Doncel (Barcelona: Seminari d'Història de les Ciències, universitat Autònoma de Barcelona, 1987), 15.

It is the trivial case of Wren's law of nature (and he explicitly depicts it as a balance). It is Huygens's second axiom (which he calls a hypothesis) and is thus stated without proof. He used it to derive other more complex cases of collision. And it is Descartes's first rule of collision (and the only Cartesian rule that Huygens did not reject). However, as shown in the previous section, the reasoning behind rules 6, 7c, and 1, have a complex relationship between equilibrium and the contest view in Descartes's impact law.[766] As we have seen in chapter 5, this notion of symmetry played a significant role in Wren's theory—working in tandem with the values he put on brevity, unity, and the economy of the algebraic symbol—in the presentation of his theory, which was organized symmetrically. The diagrams, for example, were meant to be read in both the "Latin way" as well as the "Hebraic" (from left to right, and right to left, for different instances of collision).

2.4.3 – Relativity and symmetry

In addition to the role of symmetry as a property, the latter notion of symmetry—a transformation in which something remains invariant—is of primary importance in Huygens's theory of collision.[767] Huygens's second hypothesis can be regarded as expressing symmetry of an entity. But, as shown in chapter 5, Huygens's use of

[766] Aiton claimed that laws 1 and 7c have a problematic relationship with the impact law. Rather than consequences of the law, he claims that they must have been "interpolations." 7c is an interpolation between rule 7a and 7b, and 1 is a limiting case between rules 2 and 3. With my reinterpretation of Descartes's rules, the relationship is shown to be unproblematic. See chapter 3, and E. J. Aiton, "The Vortex Theory of the Planetary Motions," *Annals of Science: a quarterly review of the history of science since the renaissance*, 13 (1957): 252.

[767] The general manner of conceptualizing collision in terms of transformations, which rely on the general structure of a relation remaining invariant amid a change, can be seen in several other accounts of collision as well. For example, Beeckman, Descartes, and Wallis all attempt in one form or another to analyze some collisions in a two-step process. In several instances in which both bodies are initially in motion, they pursue a strategy of transforming the scenario into a case in which one body is initially at rest. However, these strategies do not employ principles of relativity, and they are piecemeal.

hypothesis two, along with his relativity principle (hypothesis 3) to establish other cases of collision, should be regarded as the latter notion of symmetry. For example, as a heuristic, Huygens describes a person on a boat holding pendulum bobs in each hand. If he were to bring his hands together at a constant and equal speed, the bodies after collision would move with the same equal speeds in opposite directions (hypothesis 2). If the boat were to move at a constant speed equal to that with which the man on the boat moves one of the pendulum bobs, to a person on the shore, the pendulum bob would appear to be at rest. And if the two men were to touch hands as the man on the boat brought the pendulum bobs together in accordance with hypothesis 2, the person on the shore would affectively hold one bob at rest and move the other bob toward it. The role of relativity as a transformation to describe the invariance in collisions is central to Huygens's theory. His notion of relative motion bears some similarities with Descartes's account of motion, but was likely an extension of Galileo's relative motion.[768] Huygens's principles of relativity, however, was much more radical.

As shown in chapter 4 Huygens argued against Descartes's account of collision, from a position that initially appears to be internal to Descartes's own system. Huygens used Descartes’s first rule—that two equal bodies moving toward each other with the same speed will reflect and move away from each other with the same speed[769]—and the principle of the relativity of motion (which at least *seems* to be similar to Descartes's notion of motion) to argue that the Cartesian rules are inconsistent and that the Cartesian

[768] Vilain, "Huygens' Galilean Mechanics," 194-7. Alan Gabbey, "Huygens and mechanics," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980), 179.
[769] AT VIII 68. *Principia* II 46.

“quantity of motion” is not actually conserved.[770] However, Huygens modified fundamental components of Descartes's system, namely the conceptions of "body" and "motion." On close inspection, Huygens's understanding of motion, particularly his notion of relative motion, was *not* Cartesian.

In the *Principles of Philosophy* Descartes had described a "vulgar" and a "proper" conception of motion. The vulgar conception "is nothing other than *the action by which some body travels from one place to another.*"[771] Under this conception, Descartes shows that the same thing can be said "to move and not to move." He uses the example of a man seated on a ship. His surroundings on the ship appear to be at rest, but he and ship move away from the port.[772] But this is only motion "as commonly interpreted." Descartes's "proper" conception of motion, which is "in accordance with the truth of the matter," attributes to motion some "determinate nature." By shifting the focus from a "change of place" to the "transference with respect to the immediate neighborhood," Descartes's "proper" conception was intended to limit the arbitrariness of whether a thing is in motion or not.[773]

> [Motion] is *the transference [translatio] of one part of matter or of one body from the neighborhood [vicinia] of those bodies that immediately touch it and are regarded as being at rest, and into the neighborhood of others.*[774]

However, Descartes's also acknowledged that it is impossible to know whether the neighborhood moves with respect to the body or the body moves with respect to the

[770] Westfall, *Force in Newton's Physics,* 148-58. Compare this to Dijksterhuis's rather anachronistic and abbreviated account of Huygens’s use of relativity in *De motu corporum ex percussione*. Dijksterhuis, *Mechanization* (IV: 143-4), 374-5.
[771] AT VIII 53. *Principia* II 24. Translation by Miller, *Principles*, 50.
[772] Ibid. "Thus a man, seated in a ship which is sailing out of port, thinks that he is moving if he turns his attention to the shores, which he considers to be at rest. But he does not think so if he turns his attention to the parts of the ship, in relation to which he constantly maintains the same situation."
[773] Daniel Garber, *Descartes' Metaphysical Physics* (Chicago: University of Chicago Press, 1992), 162-72.
[774] AT VIII 53. *Principia* II 25. Translation by Garber, *Descartes' metaphysical physics*, 159-60.

neighborhood. Transference is reciprocal.[775] Some commentators have claimed that this is evidence that Descartes's proper conception of motion is ultimately relative. Garber, on the other hand, has argued that even with the "reciprocity of transfer" Descartes provided a non-arbitrary distinction between motion and rest. Motion is the mutual separation of a body and its neighborhood.[776]

Whether or not the "reciprocity of transfer" entails some form of relative motion, Descartes's conception of motion is clearly different from Huygens's. Rather than the "vulgar" notion of motion in which the observer's perception determines whether a body is in motion, Descartes defined a "proper" conception of motion, which privileged the reciprocal transfer of a body and its neighborhood. Huygens, on the other hand "believed from the beginning of his career not only that the position of the observer influenced the perception of motion, but also that there was no privileged point of view, for all viewpoints were equivalent."[777]

Moreover, whether or not Descartes's account of motion is truly relative, it is incompatible with his own rules of collision. The rules of collision appear to be compartmentalized from Descartes's refined exposition of motion. The received view is that motion is relative for Descartes. If so, the classic example of inconsistency is the lack of symmetry between rules 4 and 5.[778] But, even with a more subtle understanding of

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[775] AT VIII 55-6. *Principia* II 29. "Finally, I added that the transference take place from the neighborhood not only of any contiguous bodies, but only from the neighborhood *of those regarded as being at rest.* For that transference is reciprocal, and we cannot understand body AB transferred from the neighborhood of body CD unless at the same time body CD is also transferred from the neighborhood of body AB." Translation by Garber, *Descartes' metaphysical physics*, 166-7.
[776] Garber, *Descartes' metaphysical physics*, 168.
[777] Vilain, "Huygens' Galilean Mechanics," 196. Also see Christiane Vilain, "Huygens and Relative Motion," in *Relativity in General: Proceedings of the Relativity Meeting '93*, ed. by Diaz Alonzo and M Lorente Paramo (Gif-sur-Yvette Cedex: Atlantica Seguier Frontieres, 1995), 161-9.
[778] AT VIII 68-69. *Principia* II 49-50. Rule 4 describes a small body approaching a larger body at rest. Rule 5 describes large body approaching a smaller body at rest. If one can arbitrarily choose a reference frame to

Descartes's notion of motion (such as Garber's account), the rules remain problematic. With the latter approach, Descartes is understood to have made a meaningful distinction between motion and rest. Of primary importance to this difference is the immediate surrounding (*i.e*. neighborhood) of a part of matter. Motion is defined by the mutual separation (or transference) of the part and its neighborhood. Rest is defined by no transference between the part and its immediate surroundings. In the rules of collision, however, the two bodies are described in isolation. They are explicitly not surrounded by a neighborhood in the plenum. Thus, there is no meaningful way to describe a body at rest (according to Descartes's prior discussions), and yet three of the rules involve scenarios in which one body is at rest.

Huygens's relativity principle may have been inspired by Galileo, whom Huygens greatly admired.[779] However, Galileo's relative motion was restricted to local problems. He did not intend to make a general claim about motion and space. "[F]or Galileo, the impossibility of judging from the inside whether a boat or the Earth is in motion does not imply that such a motion does not exist. Galileo never said that the shore or the stars could be considered as being in motion just as the boat or the Earth."[780] Huygens's

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describe motion, then the initial conditions of rules 4 and 5 are equivalent. However, the conditions described after collision in rules 4 and 5 are not equivalent. The smaller body rebounds after meeting the larger body at rest in rule 4, whereas motion is transferred from the larger moving body to the smaller at rest and both move together after collision in rule 5.

[779] Gabbey, "Huygens and mechanics," 175-81. Alan Gabbey has suggested that Huygens's relativity principle did not originate with Descartes, but rather with Galileo. According to Gabbey, *Galilean* relativity involved "dynamic considerations" which Huygens too had used in his earliest work on collision. Huygens initially appealed to a force, the *vis collisionis*, in his 1654 notes on collision, and used the invariance of *forces* with respect to frames of reference in his early collision theory. Huygens would later abandon the *vis collisionis*, but it was present in his first arguments using relativity. Compare this with Westfall, *Force in Newton's Physics*, 150-1. In his chapter, "Christiaan Huygens' Kinematics," Westfall also argues that Huygens's work on collision began with a notion of force that was later abandoned. However, unlike Gabbey, Westfall claims that Huygens used Descartes's relativity principle against Descartes: "Here, of course, was the principle of the relativity of motion, Descartes' own principle turned against his own conclusions."

[780] Vilain, "Huygens' Galilean Mechanics," 196.

principle of relativity was far more radical.[781] Together with the "symmetrical case" of collision, Huygens's relative motion was a principle of symmetry at the foundation of his theory of collision.

Section 3
Mathematization

This dissertation contributes to the renewed study of the "mathematization of nature." With a historicist methodology it provides significant reinterpretations of the mathematical theories of collision of several major figures in 17th century science. And doing so, it charts the emergence of an algebraic physico-mathematics, and describes the mutual impact of algebra and collision.

The "mathematization" of nature was a classic thesis of the scientific revolution, espoused in various forms by a wide range of historians and philosophers including E. A. Burtt, Edgar Zilsel, Edmund Husserl, Alexandre Koyré, and E. J. Dijksterhuis. In general it claimed that there was a developing trend in early modern Europe—running counter to Aristotelian philosophy—that understood the underlying structure of nature to be mathematical, and a corresponding conviction that the best means of explaining fundamental aspects of nature was through mathematics.[782] Koyré, for instance, claimed

[781] Vilain, "Huygens' Galilean Mechanics," 196. Dijksterhuis, *Mechanization* (IV: 149), 378. HOC 16: 222. "True motion is relative motion."
[782] E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, New York, 1924. Revised Edition 1932 (Garden City: Doubleday & Co., Inc., 1954). Edgar Zilsel, "The Sociological Roots of Science," *The American Journal of Sociology* 47 (1942): 544-62. Alexandre Koyré, "Galileo and Plato," *Journal of the History of Ideas*, 4 (1943): 400-28. E. J. Dijksterhuis, *Mechanization of the World Picture*: *Pythagoras to Newton*, Amsterdam, 1950, trans. C. Dikshoorn and reprinted (Princeton: Princeton University Press, 1986).

that Galileo, led by a Platonist philosophy of mathematics, was the first to mathematize nature by identifying physical space and motion with corresponding idealizations from geometry.[783] Through the latter half of the 20th century, historians and philosophers of science determined several problems with this thesis. Sophie Roux, in the article, "Forms of Mathematization," has reviewed several of these criticisms, and in light of them, has proposed a renewed study of mathematization.[784] For example, there was not a rigid distinction between quantitative physics and Aristotelian philosophy. There had been mixed mathematical sciences since antiquity. Additionally, it has been questionable to assume that "motion" is the most consequential aspect of the scientific revolution.[785] And mathematical languages are not neutral. "Even more clearly than in the case of a translation from one natural language to another, the shift from one symbolic language to another entails that certain possibilities are opened while others are closed."[786] Moreover, "there was never a working definition of mathematics in general"

> [T]here were different conceptions of quantities, and consequently different ways of conceiving of the unity of mathematics. [...] [It] is a fundamentally complex field, that has included various domains from its very beginning and that has kept developing new domains throughout history.[787]

Michael Mahoney's work has also stressed this aspect of mathematics, and distinguishes in general, between geometric and algebraic modes of thought.[788] Roux proposes, "we

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783 Koyré, "Galileo and Plato," 400-28.
784 Sophie Roux, "Forms of Mathematization," *Early Science and Medicine* 15 (2010): 319-337.
785 Roux, "Forms of Mathematization," 321. Rather than focus on laws of motion, the "'relevant epistemological units' for an understanding of the Scientific Revolution are [...] a complex set of problems embodied in mundane objects."
786 Ibid., 322.
787 Ibid., 324.
788 Michael S. Mahoney, "The Beginnings of Algebraic Thought in the Seventeenth Century," in *Descartes: Philosophy, Mathematics and Physics,* ed. by Stephen Gaukroger (Sussex: Harvester Press, 1980) 141-55. Michael S. Mahoney, *The Mathematical Career of Pierre Fermat 1601-1665* (Princeton: Princeton University Press, 1973, 2nd edition 1994) 2-16. In the latter Mahoney emphasizes the diversity of mathematics in the 17th century and provides a classificatory scheme. Wallis's and Barrow's competing conceptions of mathematics have been fertile ground for case studies of conflicts between multiple forms of

should neither look for a definition of mathematics in general, nor think of mathematics as a unified field of knowledge, but rather, submit to an historically situated and empirical definition of mathematics, namely what should be called 'mathematics' is the activities of those who called themselves or were called by others 'mathematicians.'"[789] With a focus on mathematical practice, rather than "idealization," she articulates various distinct forms of mathematization including "quantification" (arithmetic), "geometrization," "axiomatization," and the application of symbolic algebra.[790]

This dissertation follows in the tradition outlined by historians such as Roux and Mahoney. My historical narrative traces the emergence of an algebraic physico-mathematics. And, using "historically situated" conceptions of mathematics, I have provided significant reinterpretations of Descartes's rules of collision as well as the development of Huygens's theory of collision.

Historians have taken care to avoid "presentism" in narratives of the conceptual development of science. However, they have been less conscientious with mathematics. The technical aspects of contemporary mathematics have been explicitly avoided, as if to focus only on the concepts of science. Treating mathematics as if it were a neutral

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mathematics in the 17th century. Katherine Hill, "Neither Ancient nor Modern," *Notes and Records of the Royal Society of London* 50/51 (1996/7): 165-78 / 13-22. and Chikara Sasaki, "The acceptance of the theory of proportions in the sixteenth century: Barrow's Reaction to the Analytic Mathematics," *Historia Scientiarum* 29 (1985): 83-116.

[789] Roux, "Forms of Mathematization," 234-5.

[790] Ibid., 325-6. Others have used a similar historicist methodology. Peter Damerow, Freudenthal Gideon, Peter McLaughline, and Jürgen Renn have emphasized the "theory of proportions" as an example of a component of "shared knowledge," in *Exploring the limits of pre-classical mechanics*, 2nd edition (New York: Springer, 2004). Niccolò Guicciardini has investigated the differences between analytic and synthetic methods, as well as the use of geometric versus analytic limits in Newton's *Principia* and his work on the motions of the moon. Niccolò Guicciardini, "Geometry, the Calculus and the Use of Limits in Newton's Principia," in *The applications of mathematics to the sciences of nature*, eds. Paola Cerrai, Paolo Freguglia, C. Pellegrini (New York: Springer, 2002), 223-32. Niccolò Guicciardini, *Isaac Newton on Mathematical Certainty and Method* (Cambridge: MIT Press, 2009). Niccolò Guicciardini, *Reading the Principia: The Debate on Newton's Mathematical Methods for Natural Philosophy from 1687 to 1736* (Cambridge: Cambridge University Press, 1999).

language, modern mathematics has been used as an interpretive tool to "clarify" the history of science. This obscures key features of the development of science. The changes that mathematics was undergoing, and the particularities of the various fields and traditions, notably impacted the development of the rules of collision, for example. The particular mathematical concepts were intrinsic to the mathematical study of nature.

Richard Westfall, for example, explained that in his history of ideas he "attempted to define the problems on which they expended themselves in their terms, and to see their proposed solutions in relation to the intellectual equipment at their disposal." However, he explicitly did not include the technical aspects of mathematics as a relevant part of his history of ideas:

> Whereas I devote no attention to social factors, I devote very little more to technical mathematical questions. I do not mean to deny in any way the importance of mathematics in seventeenth-century dynamics. With the calculus, for example, a whole new range of problems hitherto beyond the grasp of quantitative mechanics became amendable to exact treatment. My central concern has focused on conceptual issues, however; and bringing the development of dynamics up to Newton, such matters appear to me to have been central to the science of dynamics.[791]

Although the technical aspects of calculus would provide greater exactness, Westfall claimed that the concepts themselves, particularly prior to the development of calculus, were not significantly influenced by the particular form of mathematics. D. T. Whiteside, in his paper "Patterns of mathematical thought in the later 17th century," emphasized that understanding the contemporary mathematics is important in order to understand the particular "proof-structures" that were used in the 17th century. Nevertheless he "reluctantly" used anachronistic notation:

> With few exceptions historians have in the past considered it not very important to study outdated forms of proof, considering them—if at all—the subject matter

[791] Westfall, *Force in Newton's Physics*, ix-xi.

> of logic and preferring to substitute modern proofs. From the present viewpoint, however, the proof-structure is at least as important as the particular result obtained by it, and it becomes possible many times to see how the inadequacy or lack of proof-structures conditioned the development of whole classes of results. For the most part—notably in examining the method of exhaustion—where the original notations would seem to obscure ideas which can be clarified in appropriate symbolism, anachronistic notation is used. This concession to concise expression and to understanding was not made without hesitation, but rather than become involved in an intricate study of the modifying influence of symbolism it seemed preferable to substitute a cautious use of modern notation for the often unnecessarily cumbrous original.[792]

Contrary to Westfall, this dissertation has shown that the technicalities of mathematics—some of them quite simple, and in the years *before* the advent of calculus—are intrinsic to the conceptual development of science. Rather than focus on "proof structures," this dissertation has shown that changes in mathematics had an influence on the very formulation of concepts.

The reconstruction of historical ideas with modern mathematics obscures these developments. Examples of this practice could be presented for each of the figures in this dissertation. It is particularly striking in recent scholarship on Christiaan Huygens. I have argued that in 1652 Huygens's used Cartesian symbolic algebra as an innovative heuristic tool to criticize Descartes's rules of collision, and likely used the symbolic algebra as scaffolding to develop a new principle of conservation. Not only were Huygens's uses of algebra novel, but his equations are not equivalent to modern reconstructions; this is particularly apparent in Huygens's avoidance of negative numbers. Huygens's use of algebra, and the successful predictions he made while using his equations at the Royal Society, were a key step in the development of an algebraic physico-mathematics. This is eclipsed by modern reconstructions of Huygens's equations from the 1652 manuscripts as

792 D. T. Whiteside, "Patterns of Mathematical Thought in the later Seventeenth Century," *Archive for History of Exact Sciences* 1 (1661): 181-2.

well as reconstructions of his statement of conservation of quantity of motion with direction.

For example, in the 2005 biography of Huygens, Andriesse writes:

> It is rather unfair to view these pages [the "large single sheet" on which "Christiaan notes down all essential theorems for the concept of collisions" in 1652] with our present-day knowledge of physics, but we can hardly do otherwise, and it does allow us insight into his achievement. If we then attempt to view the work with the knowledge of Galileo or Descartes, then perhaps this sin could be forgiven.[793]

Andriesse goes on to "derive the laws of collision," presumably as he suspects Huygens may have done himself. However, Andriesse uses a combination of Huygens's concepts and modern algebraic equations.[794] But more importantly he is not attentive to Huygens's avoidance of negative quantities, and presumes the modern convention of directionality with positive and negative signs: "This is perhaps even clearer when numbers are used, for instance, meters per second. Let velocities to the right be positive and those to the left negative."[795] The link between positive and negative signs and the directionality of motion was in the process of emerging. In Huygens's summaries, published with the *Journal de Sçavans* and the *Philosophical Transactions*, Huygens claimed in the 5th proposition that when a contrary direction is *subtracted* from the former, Cartesian quantity of motion is conserved. However, he does not appear to have embraced the independent notion of a negative speed. Speed remained a positive magnitude in Huygens's mature work. When two equal bodies with equal speeds collide from contrary directions they rebound in opposite directions retaining their original speeds. It is not

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[793] C. D. Andriesse, *Huygens: The Man Behind the Principle* (New York: Cambridge University Press, 2005), 104.

[794] He does not use Huygens's Cartesian notation convention (*x*, *y* for unknowns, *a*, *b*, *c* for knowns), but rather uses his own convention: *M* and *m* for the "masses" of the bodies, and *B* for the initial velocity and *A* for the acquired velocity of the larger body, and *b* for the initial speed and *a* for the acquired speed of the smaller body.

[795] Andriesse, *Huygens*, 107.

until the work of Wallis and Wren that "nature obeys the algebraic laws of addition and subtraction," and directionality of motion is identified with positive and negative signs.

Similarly, Curtis Wilson's 2011 translation and commentary of Huygens's texts on "the rules for the motion of bodies arising from mutual impact" uses modern mathematics to reconstruct Huygens's ideas. The "Comments on Huygens's Summary" describe Huygens's rules in terms of the "conservation of kinetic energy" and the "conservation of momentum," and expresses Huygens's rules 5 and 6 in terms of the modern equations for those conservation principles. In an lengthy note on the commentary, Wilson et al. provide extensive symbolic derivations: "a derivation of conservation of momentum from Huygens's diagrams and his Rule 4," a "Derivation of Conservation of Kinetic Energy," as well as a "Derivation of Conservation of Velocity."[796]

Modern algebraic reconstructions are also at the heart of the longstanding interpretive challenges facing commentators of Descartes's rules of collision. The standard interpretation of Descartes's rules of collision has presented the quantity of motion as the product of body and speed. The forces involved in the impact law, such as the moving force and the force of resistance, have likewise been expressed by historians as algebraic products akin to the quantity of motion. Not only did Descartes not express these forces as products, three of Descartes's rules describe one body at rest. The above interpretation would imply that the force of resistance in the body at rest should be nil. This is an interpretive difficulty since Descartes claims that the force of resistance in the larger body at rest is greater than the moving force in rule 4, and is equal to the moving force in rule 6 (in which both bodies are the same size). The solution that historians such

[796] Gemma Murray, William Harper, and Curtis Wilson, "Huygens, Wren, Wallis, and Newton on Rules of Impact and Reflection," in *Vanishing Matter and the Laws of Motion: Descartes and Beyond*, ed. by Dana Jalobeanu and Peter R. Anstey (New York: Routledge, 2011) 160, 187-8.

as Aiton produced for this difficulty was to continue to express the force of resistance as a product, but rather than the product of the body and the speed of the body at rest, it has been argued to be the product of the body and the speed of the body approaching. I contend that this interpretation, which relies on a mathematical expression that Descartes did not use, is incorrect. I have also argued such an interpretation has led to several unnecessary interpretive problems with both Descartes's explanations in the famous letter to Clerselier as well as the French edition of the *Principles of Philosophy*. Using the received view of the force of resistance as a lens, it has been argued that there is a proliferation of new principles in the letter to Clerselier, with some, such as the Principle of Least Modal Change, bearing no connection to the impact law, and others seeming to be completely arbitrary with no justification at all. According to the received view, the subsequent French edition of the *Principles* relies on a hybrid of some of these principles in the justification of the rules of collision, but is ultimately a hastily composed unfinished draft.

If we maintain a historicist focus on the mathematics that Descartes himself used, and if we are attentive to Descartes's own guidance for understanding the rules of collision according to the outcomes of the contest between the force of resistance and moving force, we find that Descartes's rules of collision are much less problematic than has usually been alleged. There is an underlying pattern[797] and *method* unifying the rules of collision, which we see clearly when they are reorganized as I have suggested. Chapter 3 has attempted to make sense of Descartes's practice, and has recaptured the method that

[797] Chapter 3 uncovered a unifying pattern throughout the rules. There is a distinct similarity in the structure of those rules that share an outcome of the impact law—the force of resistance is not overcome resulting in rebound such as rules 4, 2, and 7b, the force of resistance is overcome resulting in a transfer of motion such as rules 5, 3, 7a, or there is a balance between the forces such as 6, 7c, 1.

was advocated and practiced by Descartes and others in the 17th century, the analytic method. I have shown that the letter to Clerselier is a well-ordered letter, in which Descartes explains himself in three interlocking ways. Rather than a hybrid of ideas in the French edition, we find Descartes revealing the analytic method to further explain how he attempted to solve the problem of collision.

Maintaining this historicist method also makes it possible to recognize the transformation in mathematization, namely the emergence of an algebraic physico-mathematics, and the connection between the development of the rules of collision and the development of algebra used in a physical context, which will be reviewed in the next section.

3.1 – Algebra and Collision.

> *Philosophy [i.e. physics] is written in this grand book—I mean the universe—which stands continually open to our gaze, but it cannot be understood unless one first learns to comprehend the language and interpret the characters in which it is written. It is written in the language of mathematics, and its characters are triangles, circles, and other geometrical figures, without which it is humanly impossible to understand a single word of it; without these, one is wandering around in a dark labyrinth.*[798]

For Galileo geometry was the language of the grand book of the universe. Its characters were triangles, circles, and geometrical figures. In general, this was the fundamental language used by Harriot (whose equations were symbolic representations of the steps of a geometric construction), Beeckman and Descartes (who relied on the magnitudes and proportions of the pseudo-Aristotelian balance), and Huygens (who formulated his theory in the axiomatic tradition of Galileo and Archimedes). However, mathematics was itself

[798] Galileo Galilei, *Il Saggiatore* (Rome, 1623). Translation by Stillman Drake, *Discoveries and Opinions of Galileo* (New York: Anchor Books, 1957), 237-8.

developing. Descartes's *La Géométrie* was an important part of these developments, and in his private manuscripts and during experiments at the Royal Society, Huygens used equations written in Descartes's symbolic algebra. Wren and Wallis continued algebraic investigations of collision. For them, algebra was not understood as a mere tool. According to Wren "nature obeys the algebraic laws of addition and subtraction." In other words, as the natural world was increasingly being investigated mathematically, mathematics itself was changing. The rules of collision are a focal point of these changes. Not only did the 17th century see a "mathematization of nature," but there were significant changes in the mathematics of nature. Specifically, developing in tandem with the rules of collision was the emergence of an algebraic physico-mathematics. This new mathematics of nature made possible new conceptualizations of the directionality of motion, and provided a unified expression of collision in a system of equations with two unknowns. The algebraic operators $+$ and $-$ provide a new expression of contrary motion, and conversely the contrary motions involved in collision would provide (in the work of John Wallis, for example) a legitimation of heretofore "impossible quantities" such as negative numbers.

Descartes's mathematics played a pivotal and ironic role in the emergence of an algebraic physico-mathematics. Beeckman investigated the apparent "destruction" of motion in the collision of bodies, using the proportions from the pseudo-Aristotelian balance. Rather than focus on the loss of motion, Beeckman's colleague, Descartes, changed the emphasis to the "quantity of motion," and put the conservation of this quantity at the heart of his system. Nevertheless, the mathematics of Descartes's early view of collision is strikingly similar to Beeckman's use of the pseudo-Aristotelian balance. Descartes made a major contribution to mathematics with the publication of his *La Géométrie*, which used the tools of symbolic algebra to analyze classic problems in geometry, such as the problem of Pappus. Descartes was influential in the development of analytic geometry; he was also responsible for framing the topic of the rules of collision. Both proved to be influential, and both would become intertwined. However, Descartes himself never brought them together. Despite the claims by some historians,[799] Descartes never intended for his analytic geometry to frame his physics, which in the *Principles of Philosophy* remained largely a "mathematical physics without mathematics."[800] The rules of collision are one exception in which Descartes's physics does contain mathematics, although only rudimentary numerical examples. Quantities are (positive) continuous magnitudes. The rules involve simple relations of quantities, which—like Beeckman's—are not unified in a single expressive system. Instead multiple mathematical strategies are used to express the various ways bodies collide. With four centuries of hindsight, we see that Descartes had nearly all the components available to him to understand collision in the way that it ultimately would be. Descartes was largely responsible for developing these tools in the first place, and yet he did not take advantage of them. This should not be seen as a failure. Rather, it is a sign of the deep significance of the differences in mathematical fields. And it indicates just how unapparent it was to

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[799] See appendix 1.

[800] Mouy, *Développement*, 144. Koyré, *Galileo Studies*, 90. According to Mouy, "La physique cartésienne est une physique mathématique sans mathématiques. C'est une géométrie concrète, ce n'est pas une géométrie analytique, une algèbra de l'univers." This was also noted by Koyré, "It is well known that Descartes' physics, as it is set out for us in the *Principes* no longer contains mathematically expressible law. It is, in fact, no more mathematical than that of Aristotle." Also see Daniel Garber, "A different Descartes: Descartes and the programme for a mathematical physics in his correspondence," in *Descartes' Natural Philosophy*, ed. Stephen Gaukroger, John Schuster, and John Sutton (New York: Routledge, 2000) 114. "The physics of the *Principia* is all words."

link together the symbolic analytic tools from Descartes's algebra with the relationships between physical magnitudes in the physico-mathematics of his natural philosophy.

Huygens acquired in-depth knowledge of Descartes's algebra (being tutored by Frans van Schooten and contributing to Schooten's Latin translation and systematization of Descartes's *La Géométrie*), modified the domain of its relevance (rather than express geometric problems and curves, he used it to examine relationship between different kinds of physical quantities), and in his early manuscripts turned it against Descartes's physics (showing that Cartesian quantity of motion is not conserved and the rules are inconsistent). The composition of these early manuscripts coincides with Huygens's first announcements of his position against Descartes.

Huygens heuristic work with algebra likely served as scaffolding for a new principle, the conservation of Cartesian quantity of motion with direction. However, Huygens did not fully replace Descartes's scalar quantity with a vector quantity. He did, however, state that the quantity of motion is conserved when motion in a contrary direction is subtracted. This amendment of Descartes's conservation principle was likely inspired through his various algebraic investigations of the quantities involved in collision.

And Huygens used symbolic algebra at the early meetings of the Royal Society to accurately predict the motions of colliding pendulum bobs, surpassing everyone else who was present. This event must have made a lasting impact on his contemporaries, and was remarked upon in letters between Moray, Oldenburg and Spinoza.[801] Individuals in

[801] HOC 5: 547. Oldenburg to Spinoza, 18 December 1665. Also see OCH 2: 624-5. Moray to Oldenburg, 27 November 1665. See chapter 4.

attendance, such as Christopher Wren and John Wallis, would go on to produce their own algebraic investigations of collision.

Despite the successes of his algebra, Huygens appeared not to be completely satisfied with his equations. They remained private. His algebraic studies are found in his manuscripts but not in the treatises prepared for publication. The latter, which are formulated in the axiomatic tradition of Galileo and Archimedes, employ geometric notions and the theory of proportions with such a thoroughness that commentators have suggested that Huygens must have *thought* in geometry.[802] Even after his successful predictions at the Royal Society, he scribbled out his algebraic equations. This may be due to Huygens's preference for the demonstrative power of classical mathematics, but it may also be due to what may have seemed to be the problematic consequences of algebra, which defied classical notions of quantity. On close inspection, we find that Huygens's algebra bears a complex position in-between modern algebra and the traditional requirements on quantity. As has been demonstrated in chapter 4, this can be seen in the great lengths to which Huygens went to avoid negative quantities in his

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[802] A. E. Bell, *Christian Huygens and the Development of Science in the Seventeenth Century* (London: St. Ann's Press, 1947), 25. H. J. M. Bos, "Huygens and mathematics," in *Studies on Christiaan Huygens*, ed. H. J. M. Bos et al. (Lisse: Swets & Zeitlinger B. V., 1980), 132. H. J. M. Bos, "Introduction," in *Christiaan Huygens' The Pendulum Clock*, trans. Richard J. Blackwell (Ames: Iowa State university Press, 1986). Joella Yoder, *Unrolling Time: Christiaan Huygens and the mathematization of nature* (Cambridge: Cambridge University Press, 1998), 172. Guicciardini, *Reading the Principia*, 119. Many have rightly commented upon Huygens's strength and preference for geometry. According to Bell, "He was hailed as the reborn Vieta and compared with Pappus and Apollonius, two giants of Greek geometry." Bos has claimed that Huygens must have *thought geometrically*. And Yoder has claimed that Huygens "regarded nature fundamentally as a geometric realm." To support her claim she cites the following: "in the *Cosmotheoros* he argues that people on other worlds would still develop Euclidean geometry because the same mathematical principles abide throughout the universe. In other words, mathematics is not an abstract construct of our earthly minds but informs nature." Yoder goes on to say that "he simply saw the physical world with the eyes of a geometer." The *Horologium oscillatorium* (1673), often called a "geometrical physics," is organized in a strict axiomatic style and employs the theory of proportions with such rigor that, with almost no exceptions, no multiplication of dimensionally different magnitudes occurs in the whole work. Guicciardini has argued that Newton used Huygens's *Horologium oscillatorium* as a model for the *Principia*, but even so, Huygens was at times critical of Newton's non-classical use of the theory of proportions.

heuristic use of equations in his manuscripts.

Thomas Harriot had used symbolic equations to represent the steps of a geometric construction, which provided the underlying structure of collision. Descartes used symbolic algebra as an analytic tool to study geometric curves. He also framed the topic of collision in the context of the laws of nature, but the mathematics of his rules of collision was not algebraic. Huygens used Descartes's analytic tool (symbolic algebra) to challenge Descartes's rules of collision. It was also employed as scaffolding for a new principle, and in the successful predictions of experiments. However, algebra remained a private heuristic for Huygens. Wallis and Wren not only expressed their theories of collision algebraically and published them, but Wallis would use the notion of contrary motion, which is central in investigation of collision to legitimize the notion of a negative number.

Many of the accounts of collision relied on traditional presumptions regarding physical quantities such as body and speed as "positive" magnitudes. This was supported by the conceptual use of the balance as a framework for the proportions among the quantities, and the representational use of geometric diagrams. In the dominant "geometric mode" of thinking, only positive constant magnitudes were used for speeds in any direction.[803] Huygens, Descartes, Beeckman, and Harriot, for example, did not use algebraic vector quantities. Rather, bodies change speed instantaneously. And the speeds of the bodies in any direction are "positive." The line segment *AC* in figure 5, for

[803] Mahoney, "Beginnings of Algebraic Thought," 142. Michael Mahoney has distinguished between what he has called the geometric mode and the algebraic mode of thought in the 17th century. The algebraic mode of thought is “characterized by the use of an operative symbolism [...] [an emphasis on] mathematical relations rather than objects [...] [and] is free of ontological commitment.” The characteristics of Greek mathematics on the other hand, “are almost diametrically opposed to those just cited" – no symbolism, an emphasis on entities rather than relations, and a dependence on physical ontology.

example, is the speed of the larger body at *A*. This line is a positive continuous magnitude just as is the line *BC*, which is the speed of the smaller body at *B*. In this example, the body at *A* is proportional to the speed *BC* as the body at *B* is to the speed *AC*. As such the bodies will have the same speeds (in the opposite direction) after meeting as they did before. The magnitudes of the lines are determined according to proportions and understood as the speed of the bodies.

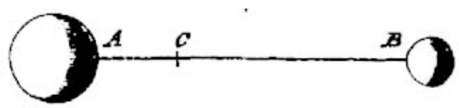

Figure 5. Christiaan Huygens, *De motu corporum ex percussione,* proposition VII, 1656

An algebraic expression, on the other hand, uses a symbolism that, in the context of a set of equations, may refer to negative velocities. In a geometric mode of thought, which according to Mahoney, relies more strongly on entities and is bound to ontological commitments, a negative speed is meaningless; in a purely algebraic mode of thinking, on the other hand, one would be free to operate with negative quantities. Whereas Huygens's equations in the manuscripts have an idiosyncratic in-between status bridging both traditional conceptions of quantity and the flexibility of symbolic algebra and avoided negative numbers, Wallis explicitly embraced the positive and negative signs to indicate the direction of motion. Not only did this extend the symbolic algebraic physico-mathematics, Wallis would later appeal to the directionality of motion in his *Treatise on Algebra* to provide a legitimation of the otherwise "impossible" negative number.

Rather than reject or restrict "impossible" quantities that could be produced from algebraic equations, Wallis openly suggested that mathematics itself should be changed to accommodate them. Since they defied classical rules of mathematics, he could not provide arguments from within mathematics. Instead he appealed to notions such as usefulness. He acknowledged the impossibility of a "quantity smaller than nothing," but argued that if one *supposes* that the clearly impossible quantity were not impossible, then one would find instances in which it was useful. A negative number could be useful in describing the directionality of motion, just as the square root of a negative number could be useful in describing the length of the side of a square of land that is lost to the sea.

With Wren, the transformation from a geometric to algebraic language of nature is completed. Wren was present when Huygens successfully used algebraic equations to predict the motions of colliding pendulum bobs in 1661. Like Wallis, he was a student of William Oughtred's algebraic mathematics. Oughtred was an advocate of the brevity and immediacy of symbolic equations. It was not merely an analytic tool to solve geometric problems like a "trick." Rather, according to Oughtred, symbolic algebraic equations laid bare the internal structure of mathematics. In Oughtred's studies of the ancient authors, he rewrote the tenth book of Euclid's *Elements* from its "ponderous rhetorical form into that of brief symbolism."[804] Wren's paper on collision for the *Philosophical Transactions*, the *Lex naturae de collisione corporum*, used the same notation and algebraic conventions found in Oughtred's works. It also shares Oughtred's characteristic ideal of brevity and, through the use of symbols, it is meant to be presented so that one can grasp "meaning at a glance."

There is an interesting parallel between the development of the theories of collision and the theory of equations, particularly the role of symbolism, which illustrates the gradual change from a plurality of rules to unified, generalized expressions.

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[804] Florian Cajori, *William Oughtred: A Great Seventeenth-Century Teacher of Mathematics* (Chicago: The Open Court, 1916), 28.

Throughout much of the history of algebra equations were classified into types, and each type had its own rules for solution. Al-Kwārizmī's *Al-jabr* (c. 825), for example classified equations into six types.[805] After Leonardo Pisano included al-Kwārizmī's rules for the six types of equations in his *Liber abaci* in 1202, algebra spread throughout Europe and "by the sixteenth century al-Kwārizmī's rules [...] were available in printed textbooks in Spain, France, Germany, and England."[806] Girolamo Cardano, despite having successfully solved cubic and quartic equations in his *Ars magna* (1545), did not use a symbolism, and used only positive coefficients. "[He] and his contemporaries referred to the unknown as the "thing" (*res* in Latin, *cosa* in Italian, and *coss* in German) and subsequent multiples of the "thing" as the "square" and the "cube.""[807] The modern general quadratic, cubic, and quartic equations "did not exist" for him. Equations, which we would now express as follows:

$$x^2 = ax + b$$

$$b = x^2 + ax$$

were understood "as distinct equations rather than equations that could be reduced to a common form. For Cardano, then, there were five basic types of quadratic equations:"

1. the square is equal to a number
2. the square is equal to the first power,
3. "the square is equal to the first power and constant,"
4. "the number is equal to the square and first power,"
5. "the first power is equal to the square and number."

[805] Jacqueline A. Stedall, *Mathematics Emerging: A Sourcebook 1540-1900* (New York: Oxford University Press, 2008), 29. "squares equal to roots, squares equal to numbers, roots equal to numbers, squares and roots equal to numbers, squares and numbers equal to roots, and roots and numbers equal to squares"
[806] Stedall, *Mathematics Emerging*, 31.
[807] Helena M. Pycior, *Symbols, Impossible Numbers, and Geometric Entanglements: British Algebra through the Commentaries on Newton's* Universal Arithmetick (New York: Cambridge University Press, 1997), 13-4.

"He directed his readers that equations of type 1 had two roots, and he gave a specific algorithm for each of the other specific types."[808] François Viète's *Introduction to the Analytic Art* was a turning point in the history of algebra. He initiated the use of arbitrary symbols instead of words and abbreviations, although in practice he used a mixture of symbols and abbreviations.[809] The next generation of mathematicians, such as William Oughtred, Thomas Harriot and René Descartes consistently used symbolic notation.[810] The symbolism did not just excise the prolixity of rhetorical algebra. The symbol had a meaning independent of the object symbolized; it was not necessarily bound by preconceptions about quantity (*e.g.* it did not matter if the quantity was a whole number or an integer, and eventually if it was negative or the root of a negative). In the terminology of Viète and the 17th century, symbolic algebra was a "logistic of species."[811] And secondly, symbolism allowed for the transformation of equations into other equivalent forms. The plurality of rules for the solution of various kinds of

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[808] Pycior, *Symbols*, 14. "For the solution of an equation of type 3, for example, he instructed readers to "add the square on one-half the coefficient of the first power to the constant of the equation and take the square root of the whole. To this add one-half the coefficient of the first power, and the sum is the value of *x* [the thing]."

[809] Pycior, *Symbols*, 27-9. Tobias Dantzig, *Number: The Language of Science*, 1st edition, New York, 1930 (New York: Plume, 2007), 81-2. Historians often describe three stages of development in algebra: rhetorical, syncopated, and symbolic. Rhetorical algebra is written entirely in prose. Syncopated algebra abbreviated commonly used words. Symbolic algebra uses arbitrary symbols.

[810] Pycior, *Symbols*, 28-9. "For example, he used abbreviations to denote powers. He wrote the square of the unknown variously as *A quadratum*, *A quad,* or, in the extreme, *Aq.* A later generation of algebraists would develop the more consistently symbolical notion for powers that is used in modern algebra. For example, the English mathematician Thomas Harriot would let *aa* stand for the square of the unknown, *aaa* for the cube, and so on; in 1637 Descartes would introduce the nearly modern notion for exponents, writing *xx*, x^3, and so on."

[811] Mahoney, *Fermat*, 36. "Unlike numerical logistic, Viète pointed out in Chapter IV of the *Introduction*, the analytic art constituted a logistic of species, an arithmetic 'set forth in terms of the species or forms of things, such as the letters of the alphabet.' The species Viète had in mind was the species of quantity [...] In the *Introduction to the Analytic Art* [...] algebra was transformed from a sophisticated sort of arithmetical problem-solving into the art of mathematical reasoning itself, insofar as that reasoning was based on combinatory operations."

equations became unified by the new generalized equations, made possible by the arbitrary symbols.[812]

The theories of collision also gradually changed from a plurality of rules and strategies to a unified account, some expressed in symbolic equations. As we saw in chapters 2 and 3, Beeckman and Descartes relied on a set of principles, but each used a variety of techniques to determine the outcomes of collisions, depending of the particular scenario. For example, unique strategies were used whether one body is at rest or both are in motion, whether one body is larger or if they are the same size, whether they move in contrary directions or the same. Whereas with Huygens, all collisions (of perfectly hard bodies) are explained with two principles: the conservation of motion with direction and the conservation of *vis viva*. And, with Wren, every scenario of collision is expressed with one "law of nature."

Wren was taught Oughtred's mathematics by his teachers Seth Ward and Charles Scarburgh. And both Wren and Ward would follow in the footsteps of Oughtred by rewriting, not Euclid's *Elements*, but rather Galileo's *Discorsi* in symbolic algebra.[813] Galileo claimed that one would be left wandering in a dark labyrinth without knowing how to read the geometrical characters of the book of nature. Wren understood that his symbolic algebra was not merely a tool to read the geometrical language. Rather his symbolic algebra was intended to lay bare the underlying mathematical structures. This had been the intention in the algebraic translations of Galileo's book. This was the purpose of Wren's symbolic algebra for the book of nature as well. A new algebraic physico-mathematics had emerged in tandem with the rules of collision. Wren concluded

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[812] Dantzig, *Number*, 89.

[813] Renée Jennifer Raphael, "Galileo's *Discorsi* as a tool for the analytical art," *Annals of Science* (2014): 1-25.

his *Lex naturae de collisione corporum* (The Law of Nature in the Collision of Bodies) with the declaration that "Nature obeys the Algebraic laws of Addition and Subtraction" (*Natura observat regulas Additionis & Subductionis Speciosæ*).

Appendix 1
A brief history of the histories of Descartes and collision, 1847—present

Historians of science have long-held several commitments regarding the importance of the mathematical studies of collision in the 17th century. Perhaps foremost among them, the studies were taken to be important because of their apparent connection to the development of laws of nature. In the second volume of *The History of the Inductive Sciences* (1847), for example, William Whewell thought that they led to the third law of motion.[814] "In its most general sense" this law states that "the quantity of motion remains unaltered, *quantity of motion* being used synonymously with *momentum*."

> [M]omentum (which is proportional to the mass of the body and its velocity jointly,) may be taken, the measure of the effect; so that this momentum is as much diminished in the striking body by the resistance it experiences, as it was increased in the body struck by the impact. This was sometimes expressed by saying that the quantity of motion remains unaltered, *quantity of motion* being used synonymously with *momentum*. Newton expressed it by saying that "action and reaction are equal and opposite," which is still one of the most familiar modes of expressing the third law of motion.[815]

Here we have another of the commitments. The studies of collision have been thought to in some way "look forward to" or imperfectly express the notion of *momentum* and its *conservation*. In another of Whewell's statements of the law we find seeds of an additional theme: He writes: "for the same body, the dynamical effect of force is as the statical effect" in other words, as he puts it:

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[814] William Whewell, *The History of the Inductive Sciences, from the Earliest to the Present Time*, vol. 2 (London: Parker, 1857) 56. "The third law of motion was still in some confusion when Galileo died, as we have seen. The next great step made in the school of Galileo was the determination of the laws of the motions of bodies in their direct impact, so far as this impact affects the motion of translation." The third law to which Whewell refers is the law of motion listed third by Descartes in the *Principles of Philosophy* (but not *The World*, where it is listed second) and by Newton in the *Mathematical Principles of Natural Philosophy*, and everyone else thereafter.

[815] Whewell, *History of the Inductive Sciences*, 57-8. "In these solutions, we perceive that men were gradually coming to apprehend the third law of motion in its most general sense."

> [T]he velocity which any force generates in a given time when it puts the body in motion, is proportional to the pressure which the same force produces in a body at rest.[816]

In Whewell's assessment, people had confused and conflated the principles of dynamics and statics in the 17th century,[817] which are the very two components of Whewell's statement of the third law—the velocity produced by the ("dynamical") force of momentum and the ("statical") pressure.[818] In part, this was due to "the close analogy and connexion which exists between the principles of equilibrium and of motion" which "often led men to confound their evidence" and resulted in ambiguity in words such as momentum and force.[819] According to Whewell, upon clarifying this distinction, the obstacle was removed and the third law became known.[820] Thus we find the beginnings

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[816] Ibid., 47.
[817] Ibid., 45, 56.
[818] Society for the Diffusion of Useful Knowledge, "Pressure," *The Penny Cyclopaedia of the Society for the Diffusion of Useful Knowledge: Peru-Primates*, vol. 18, ed. Charles Knight (London: Charles Knight and Co., 1840), 502. The article in the *Penny Cyclopaedia* on pressure, which happens to cite one of Whewell's works, defines *pressure* as it is used here: "...the notion of motion caused or prevented." The article elaborates on this point: "Whenever we see motion caused, prevented, or altered, we are apt to carry with us the notion that pressure is exerted. The weight in the scale of a balance is said to press the scale; not that we suppose the scale to have muscles to be acted upon, and nerves to carry news of the action to a living brain, but that we see a counteraction of the known tendency of the weight to fall, and know that if the counteraction were the work of a human agent, that agent would be conscious of the perception of pressure. Hence everything fitted to produce the sensation of pressure, such as a weight, the elasticity of a spring, &c., comes to be called a pressure, and the word loses its meaning of a perception conveyed, and takes that of an agent proper to produce that perception if the human being were situated so as to receive it."
According to the article, pressure is explicitly linked to the Newtonian concept "force." Pressure also explains why a hammer rebounds when it hits an anvil, which takes place continuously in a short amount of time and not instantaneously: "[When] the contact begins both hammer and anvil begin to be compressed. [...] But the resistance to compression is enormous, and is a pressure which, though it takes time to destroy any velocity, yet will destroy [the velocity] in a very small fraction of a second. The moment the velocity is all destroyed the effort of the anvil and hammer (both of which are compressed) endeavouring to restore themselves, the continuation in fact of the pressure which destroyed the velocity, will give a velocity to the hammer in a contrary direction, or the hammer will rebound, as it is well known to do. In the appendix to Professor Whewell's 'Elementary Treatise on Mechanics' (third or fourth edition), a mathematical investigation of such problems (by Mr. Airy) will be found..."
[819] Whewell, *History of the Inductive Sciences*, 58.
[820] Ibid., 34. For Whewell the principles of a "complete science" have "absolute universality and necessity." His history of science in the seventeenth century, or at least the "Inductive Epoch of Galileo," centers around the discovery of these laws. In this case the absolutely universal and necessary third law was discovered once the obstacle obscuring the distinction between statics and dynamics was removed.

of a theme that emphasizes and analyzes the connection between static principles of equilibrium and dynamic forces involved in studies of motion in the 17^{th} century and the place of "force" in the studies of collision.

Regarding Descartes's role in particular in the mathematical study of collision, Whewell's position is one that has become quite familiar to historians of early modern science: "The laws of the mutual impact of bodies were erroneously given by Descartes in his *Principia*."[821] But Whewell did not stop there. He found little of value in Descartes work on the topic:

> We may here notice Descartes and his Laws of Motion, the publication of which is sometimes spoken of as an important event in the history of mechanics. This is saying far too much. The *Principia* of Descartes did little for physical science. His assertion of the laws of motion, in their most general shape, was perhaps an improvement in form; but his third law is false in substance. Descartes claimed several of the discoveries of Galileo and others of his contemporaries; but we cannot assent to such claims, when we find that, as we shall see, he did not understand, or would not apply, the laws of motion when he had them before him. If we were to compare Descartes with Galileo, we might say, that of the mechanical truths which were easily attainable in the beginning of the seventeenth century, Galileo took hold of as many, and Descartes of as few, as was well possible for a man of genius.[822]

As we will see, later historians—while acknowledging the so-called errors in Descartes laws—take Descartes to be incredibly important in the mechanization and "mathematization of nature."[823] Whewell, however, makes no mention of this. Moreover, he deliberately does not discuss the actual mathematics used by those in the so-called "School of Galileo" such as Wren's, Wallis's, and Huygens's mathematical studies of collision, and certainly not Descartes's. In fact, Whewell claims that "[i]t is not necessary

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[821] Ibid., 57.
[822] Ibid., 50-51.
[823] E. J. Dijksterhuis, *Mechanization of the World Picture*: Pythagoras to Newton, Amsterdam, 1950, trans. C. Dikshoorn and reprinted (Princeton: Princeton University Press, 1986). E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, New York, 1924. Revised Edition 1932 (Garden City: Doubleday & Co., Inc., 1954).

for us to trace the progress of purely mathematical inventions."[824] Unlike later advocates of the "mathematization of nature" as the defining feature of early modern science and perhaps even the scientific revolution, Whewell explicitly claimed that "mathematical reasoning" was not the most essential part of science, and meant to demonstrate this claim.[825]

While Whewell de-emphasized mathematics, Ernst Mach was not shy about presenting fairly sophisticated mathematics on the topic of early modern mechanics. However, Mach used mathematics modern to his own time and was not sensitive to the mathematical concepts available to the historical actors themselves.[826] This is fitting for his project, particularly *The Science of Mechanics: a critical and historical account of its development* (1883, English translation 1893), which is not strictly historical, but rather is an attempt to show that the sometimes-confused and imperfect truths of the historical actors (expressed in latter-day form) can be explicitly deduced from later more fundamental and clearer truths closer to Mach's time.[827] Mach discusses "the laws of

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[824] Whewell, *History of the Inductive Sciences*, 53.

[825] William Whewell, *The Philosophy of the Inductive Sciences, Founded upon their History*, vol. 1 (London: Parker, 1847), 163. "[I]t may be seen how important an office in promoting the progress of the physical sciences belongs to mathematics. Indeed in the progress of many sciences, every step has been so intimately connected with some advance in mathematics, that we can hardly be surprised if some persons have considered mathematical reasoning to be the most essential part of such sciences; and have overlooked the other elements which enter into their formation. How erroneous this view is we shall best see by turning our attention to the other Ideas besides those of space, number, and motion, which enter into some of the most conspicuous and admired portions of what is termed exact science; and by showing that the clear and distinct development of such Ideas is quite as necessary to the progress of exact and real knowledge as an acquaintance with arithmetic and geometry." Also see H. Floris Cohen, *The Scientific Revolution: A Historiographical Inquiry* (Chicago: The University of Chicago Press, 1994), 33.

[826] For example Mach uses an algebra that employs negative quantities, zero, and absolute values none of which were in use in the historical period under investigation.

[827] This is not to say that Mach was not interested in history. In fact he had high praise for history: "He who calls to mind the time when he obtained his first view of the world [...] will surely remember how upside-down and strange things then appeared to him. [...] There are two ways of reconciling oneself with actuality: either one grows accustomed to the puzzles and they trouble one no more, or one learns to understand them by the help of history and to consider them calmly from that point of view. [...] [D]ifficulties lie in wait for us when we go to school and take up more advanced studies, when propositions which have often cost several thousand years' labour of thought are represented to us as self-evident. Here

impact" and claims that they were "the occasion of the enunciation of the most important principles of mechanics, and furnished also the first examples of the application of such principles."[828] These "most important principles" include the principle of *vis viva* (the living force, anachronistically expressed as mv^2), as well as "Newtonian principles." Mach focused on *experimental* attempts to ascertain the "laws of impact"[829] and highlighted the much neglected work of Marcus Marci in this regard. Notably, Mach does *not* mention Descartes's rules of collision, perhaps because there is no mention of their experimental investigation in the *Principia*. In the discussion of Wren, Wallis, and Huygens, on the other hand, he emphasizes the experiments associated with the theorems.

Unlike Whewell, the significance of impact is not its connection to the third law specifically. Rather, it as an "occasion of the *enunciation*" of important principles of mechanics. Even though "the investigation of the laws of impact contributed, it is true, to the discovery of Newton's laws" they "do not rest solely on this foundation."[830] Instead, according to Mach, the partially confused, unrecognized, implicitly assumed principles such as the conservation of *vis viva* (mv^2) merely became more clearly and explicitly enunciated. As an example, Mach analyzes Huygens's work on impact. He attempts to show that the conservation of *vis viva*,[831] although asserted as one of Huygens *last*

too there is only one way to enlightenment: historical studies. [...] Perhaps the following lines will also show the value of the historical method in teaching. Indeed, if from history one learned nothing else than the variability of views, it would be invaluable. Of science, more than anything else, Heraclitus' words are true: 'One can not go up the same stream twice.' Attempts to fix the fair moment by means of textbooks have always failed. Let us, then, early get used to the fact that science is unfinished, variable." Ernst Mach, *The History and Root of the Principle of the Conservation of Energy* (Chicago: Open Court, 1909), 15-17.

[828] Ernst Mach, *The Science of Mechanics: a critical and historical account of its development* (Chicago: Open Court, 1893), 305.

[829] Mach, *The Science of Mechanics*, 305-30. See chapter III, "The extended application of the principles of mechanics and the deductive development of the science," part IV, "The Laws of Impact."

[830] Ibid., 317.

[831] It should be noted that Huygens does not use the words "*vis viva*" at all in his 1669 paper, but instead refers to the conservation of "body and the square of speed."

theorems, as Mach himself points out, is "unmistakably at the foundation of the previous theorems."[832]

Mach's critical comments on Descartes[833] are just as severe as Whewell's. Although Descartes sought after "a more universal and fruitful point of view in mechanics" which is worthy of "merit," Descartes had several "defects."[834]

> Descartes, however, was infected with all the usual errors of the philosopher. He places absolute confidence in his own ideas. He never troubles himself to put them to experiential test. On the contrary, a minimum of experience always suffices him for a maximum of inference. Added to this, is the indistinctness of his conceptions. Descartes did not possess a clear idea of mass. It is hardly allowable to say that Descartes defined *mv* as momentum... Descartes's greatest error, however,—and the one that vitiates all his physical inquiries,—is this, that many propositions appear to him self-evident *à priori* concerning the truth of which experience alone can decide.

According to Mach, Descartes's defects are (1) no experiential tests of his ideas, (2) his conceptions, such as quantity of motion, were indistinct, and (3) he took several truths to be self-evident and did not provide evidence of their truth from experience. As an example, Mach singles out Descartes's notion that "a body preserves unchanged its velocity and direction" and finds fault with the argument for it. Descartes treats the notion, which Mach calls the law of inertia, as self evident and uses experiences "as a confirmation of an *à priori* law of inertia." What Mach thought Descartes should have done was to cite the experiences "as a foundation on which this law in an empirical sense should be based."[835] Mach thought these three defects spoiled all of Descartes work. Elsewhere he makes a similar comparison with Galileo as Whewell did, and comes to an

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[832] Mach, *The Science of Mechanics*, 317.
[833] Ibid., 272. Descartes is mentioned in the context of the history of the ideas of "quantity of motion" and "*vis viva.*" See chapter III, "The extended application of the principles of mechanics and the deductive development of the science," part II "The Formulæ and units of mechanics," section IV.
[834] Ibid., 273-4.
[835] Ibid., 274.

even stronger conclusion. Descartes's work (at least on freely falling bodies) was insignificant and would be completely effaced.[836]

In an earlier work by Mach, *The History and Root of the Principle of the Conservation of Energy* (1872, English translation 1909), he dedicates a chapter "On the History of the Theorem of the Conservation of Work." He expresses the theorem in what he considers to be two equivalent forms:

1.) $\frac{1}{2}\sum mv^2 - \frac{1}{2}\sum mv_o^2 = \int \sum(Xdx + Ydy + Zdz)$; and
2.) It is impossible to create work out of nothing, or to construct a *perpetuum mobile.*[837]

One might expect that the studies of collision in the 17th century would be an important topic in the history of the conservation of work, particularly given the conservation of mv^2 in Huygens's work. Here, surprisingly, collision plays an even smaller role than it did in the *Science of Mechanics*. Like the previous text there is no mention of Descartes's rules of collision. But here the mathematical study of collision plays *no* role at all! As was already mentioned, later historians would link Descartes to mechanization and mathematization, linking collision to the mechanical view of nature—all change occurs through the material contact of matter in motion. Even if Mach would have acknowledged these connections, Mach would probably still have maintained that Descartes and collision would have had little to do with the theorem of the conservation of work. This is because Mach thought the conservation theorem *predated* a mechanical view of the world. Moreover, it should *not* be "considered as the flower of the mechanical view of the world."[838] Rather,

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[836] Ibid., 250. "Descartes elaborated Galileo's ideas after a fashion of his own. But his performances are insignificant compared with those of Newton and Huygens, and their influence was soon totally effaced."
[837] Mach, *The History and Root of the Principle of the Conservation of Energy*, 19.
[838] Ibid., 20.

> "its logical root is incomparably deeper in our mind than that view. ... [It] is by no means so new as one tends to believe; ... indeed, almost all eminent investigators had a more or less confused idea of it."[839]

The principle of excluded perpetual motion (one of the two equivalent forms of the theorem) "cannot be founded on mechanics, since its validity was felt long before the edifice of mechanics was raised." Mach claims that long held ideas, which would eventually come to be this theorem, served as the foundation for "the most important extensions of the physical sciences."[840] But even here, instead of showing how this foundation shaped the rules of collision or perhaps the third law, Mach points to other examples: the stasis of a cord of bodies slung around a triangular prism from Simon Stevinus's *De statica*, and the notion that a descending body will rise no more than the same vertical height from Galileo's *Dialogo*, and Huygens's generalization of this principle in *Horologium oscillatorium, pars secunda*.[841] To put this in terms similar to *The Science of Mechanics*, these examples from mechanics are "occasions of the enunciation" of until then partially unrecognized, implicitly assumed principles.

In these early histories of early modern science we find a de-emphasis on the studies of collision in the history of mechanics (Mach) and no mention of it at all in the history of the conservation of work (Mach). Although, at least according to Mach, the studies would become an "opportunity for the enunciation" of *vis viva*. The histories make a connection between collision and the third law (Whewell), as well as momentum (Whewell and Mach), and the 17th century relationship between statics and dynamics (Whewell). No attention was given to the specific mathematical notions and methods of the time—Whewell de-emphasized mathematics altogether and Mach used modern

[839] Ibid.
[840] Ibid., 41.
[841] Ibid., 28.

mathematics. The normative assessment of Descartes's work in particular in these early histories is negative and dismissive. Although his laws took a general shape (Whewell) or at least sought after a universal form (Mach) which was considered to have some merit, his laws of motion are assessed as false in substance and the laws of impact in particular are erroneous (Whewell). In their evaluation, Descartes's work pales in comparison to Galileo's (Whewell and Mach), and was generally insignificant (Mach). And Descartes's vision of science was seen as defective: he did not use, or propose to use, experience adequately to test one's ideas, and he assumed that science could rest on "self evident" *a priori* truths (Mach).

Between the years 1897 and 1913 the eleven volumes[842] of the *Oeuvres de Descartes* were published, edited by the historian of modern philosophy Charles Adam and the historian of mathematics Paul Tannery.[843] The "Adam-Tannery" has been enormously influential on the history of science in the seventeenth century, and it generated new interest in Cartesian philosophy.[844] While in the midst of the editorial project, Paul Tannery had proposed a study on Descartes's seven rules of the impact of bodies, but it remains unfinished because Tannery died while at work on it.[845] The incomplete study was nevertheless published as "Sur les règles du choc des corps d'après Descartes" in the second part of the ninth volume (1904) of the *Œuvres de Descartes*.[846]

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[842] A twelfth volume, *Vie et oeuvres de Descartes* (Paris, 1910) written by Charles Adam, was included in the first edition of the project as a supplement. It was not included, however, in the second edition.
[843] Paul Tannery was involved with volumes I-VII and IX. He died in 1904 before the completion of the editorial project on Descartes's works.
[844] René Taton makes the claim regarding the renewed interest in Cartesian philosophy in the "Tannery, Paul" article in *DSB* 13: 251-6.
[845] AT VIII xiii. Charles Adam's words on the subjects of Tannery's death and his study on collision are as follows: "Paul Tannery avait été, malheureusement, interrompu, en plein travail, par la mort."
[846] AT IX part 2, 327-330.

In it Tannery's focus is on the *error* of Descartes's third law and subsequent rules of impact. Elsewhere, specifically in the editorial footnotes on the third law as found in the 1647 French translation of the *Principles of Philosophy*,[847] Tannery noted that whereas the first two of Descartes laws are considered as "scientifically acquired truth,"[848] the third law had been shown to be incorrect since the seventeenth century by the work of Huygens. "It is on this point [Descartes's false third law] that the principle error of Descartes's physics is born, an error which mars above all the rules given in articles 46 through 52 [*i.e.* Descartes's rules of collision]."[849]

The stated purpose of Tannery's unfinished study was to "indicate with precision how the seven Cartesian rules, relative to the impact of bodies, differ from the theoretical rules from Mechanics applicable to the same cases."[850] In other words, he wanted to show to what extent Descartes's rules deviated from corresponding rules derived from the theorems of rational mechanics accepted at Tannery's time. The theoretical rules from mechanics, which would serve as the standard by which to measure these differences, would be deduced "from the theorem of *la conservation du mouvement du centre de gravité* (here supposed fixed), and from *la conservation des forces vives*, demonstrated in rational Mechanics for every isolated system."[851] Tannery used the same letters (B and

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[847] AT IX part 2, 1-325.
[848] This is contrary to Mach's position.
[849] AT IX part 2, 86n. "Tandis que les deux lois prédédentes sont aujourd'hui considérées comme des vérités scientifiquement acquises, la troisième a été ruinée, des le xviie siècle, par les travaux de Huygens sur le choc des corps. C'est sur ce point que porte la principale erreur de la physique de Descartes, erreur qui entache surtous les règles données dans les articles 46 à 52 ci-aprés."
[850] AT IX part 2, 327. "Il m'a paru utile d'indiquer ici avec précision en quoi les sept règles cartésiennes, relatives au choc des corps, diffèrent des règles théoriques de la Mécanique applicables aux mêmes cas (corps parfaitement durs, isolés de tous autres, et n'ayant d'actions réciproques qu au moment du choc se mouvant enfin suivant la droite qui joint leurs centres de gravite, cette droite passant d'ailleurs par les points qui viennent en contact)."
[851] Ibid. "Ces règles théoriques sont comprises sous une formule unique qui se déduit du théorème de la conservation du mouvement du centre de gravité (ici supposé immobile), et de celui de la conservation des forces vives, démontrés en Mécanique rationnelle pour tout système isole."

C) as Descartes, but referred to *masses* rather than "bodies" as Descartes did. Along with b, c, β, and γ, which Tannery uses for the velocities of the masses before and after collision, Tannery used the letters as variables in a system of algebraic equations.[852] There is no evidence that Descartes ever expressed collision in this manner or conceptualized it algebraically.

The practice of using modern algebraic equations to represent modern physical principles (*e.g.* the equations for the conservation of momentum and the conservation of energy) in order to produce rules that are supposed to correspond to Descartes's rules, has been persistent in the history of science until comparatively recently. This practice may be effective in showing how a modified version of Descartes's ideas, which are expressed in a form quite foreign to Descartes's, compare to contemporary ideas. But this practice is not effective in revealing much about Descartes's ideas, or even how Descartes's ideas compare to contemporary ideas. Important notions, such as speed, velocity, body, mass, force, momentum, conservation, and the possible relationships between physical quantities, are affected by the manner in which they are expressed. When they are expressed mathematically, the specific mathematical concepts and methods that are involved have an effect on the understanding of the historical notions.

In 1929 the Société Hollandaise des Sciences published the sixteenth volume of the *Œuvres complétes de Christiaan Huygens*, which focused on the topics of percussion, the question of absolute motion, and Huygens's work in statics and dynamics. Similar to Adam and Tannery, the editors of Huygens's works consistently used modern mathematics to represent Huygens's ideas. Since Huygens famously challenged

[852] The equations of the two theorems, from which Tannery would produce equations of rules of collision corresponding to Descartes's 7 rules of collision, were expressed as follows:
(1) $\mathrm{B}b + \mathrm{C}c = \mathrm{B}\beta + \mathrm{C}\gamma$ (2) $\mathrm{B}b^2 + \mathrm{C}c^2 = \mathrm{B}\beta^2 + \mathrm{C}\gamma^2$

Descartes's rules of collision, much of the historical and editorial work on Descartes in the volume focuses on Descartes's "errors." For example, the index of the volume has a lengthy set of references under the heading "*Erreurs de Descartes*" with subheadings such as *Inexactitude de son assertion sur la conservation dans l'univers de la même quantité de mouvement*; *Erreurs dan la dynamique;* and *dans la théorie de la percussion*.[853]

The attitude on Descartes's errors found in volume 16 of Huygens's works had changed from the previous histories. All of Descartes's rules are reproduced from the 1647 French translation[854] in the introduction of the 16th volume. Immediately following the rules, the editor, applied mathematician and historian of mathematics Diederik Korteweg, commented on how clearly Descartes's rules of collision (particularly the 4th) "are in contradiction with the most simple experience."[855] However, Korteweg did not immediately dismiss the rules. Rather he noted that Descartes's rules "become a little less incomprehensible" when one realizes that Descartes considered them as *theoretical* rules and not rules realized in practice. He even quoted Descartes's own words to clarify the point that the bodies in the rules are to be considered without any other surrounding bodies such as air or a liquid.[856] It is notable that Christiaan Huygens's criticism of

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[853] HOC 16: 587.
[854] HOC 16: 4-5n.
[855] Volume 16 had been started by Korteweg, but, like Tannery, his death prevented him from completing the work. He was the lead editor from 1911 through 1927 and was responsible for pages 1-186, 202-212 of volume 16. J. A. Vollgraff oversaw its completion as well as the remaining volumes of the project. Notably, E. J. Dijksterhuis also worked on volume 16 (pages 344-349, 392-412, 463-469). It is Huygens's work on musicology in the first part of volume 20 to which Dijksterhuis contributed the most. See HOC 22: 816.
[856] HOC 16: 5n. "Il est clair que plusieurs de ces règles, nommément la quatrième, sont en contradiction avec la plus simple expérience. Seulement le fait de leur admission par Descartes devient un peu moins incompréhensible par ce qu'il considérait ces règles comme des règles théoretiques qui ne se réalisent dans la pratique que très rarement. Ainsi il dit à propos de la quartriéme que pour sa réalisation, il est nécessaire que le corps C "non seulement n'eust point de mouuement apparent, mais aussi qu'il fust point enuironné

Descartes's rules did not rely on the position that the latter's rules contradicted experience. Rather, Huygens used theoretical and mathematical arguments to show that Descartes's rules were inconsistent and incorrect. Korteweg's familiarity with Huygens's published and unpublished works, along with the impact of the Adam Tannery, might begin to explain his relative patience with Descartes's rules.

A few years earlier in the United States E. A. Burtt's *The Metaphysical Foundations of Modern Physical Science* (1924) was published, which, regardless of the truth value of Descartes's laws and rules of collision, placed Descartes as a key figure in what Burtt saw as a dramatic shift to the modern view of the world and humanity's place in it. Part of this shift involved what has come to be known as the "mathematization of nature." Descartes was presented as important because he "worked out a comprehensive hypothesis in detail of the mathematical structure and operations of the material universe."[857] Burtt claimed that just as Descartes had shown the relationship between arithmetic and geometry in his analytic geometry, "one not unnatural result of this notable invention" was that "the whole realm of physics might be reducible to geometrical qualities alone."[858] And thus mathematics would be "the sole key needed to unlock the secrets of nature."[859] Burtt indicated that Descartes's early project, *Regulae ad Directionem Ingenii* (composed in 1629, but posthumously published in 1701) seemed to provide this very "mathematical method." But, according to Burtt, Descartes made a wrong turn when he attempted to explain phenomenal qualities and motions by means of vortices in the plenum. Nevertheless, the assessment of Descartes's influence in bringing

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d'air, ni d'aucuns autres corps liquides, lesquels, comme je diray cy-apres, disposent les corps durs du'ils enuironnent, à pouuoir estre meus fort aisement" et il revient encore plus d'une fois...sur cette restriction..."
[857] Burtt, *Metaphysical Foundations*, 105.
[858] Ibid., 106.
[859] Ibid., 105.

about (the metaphysical foundations of) modern science is quite affirmative in Burtt's work, particularly when compared to the preceding historians of science. The emphasis was placed on Descartes's proposal that the structure of the material world is mathematical, and that mathematics is the key to knowing this world. In addition, Burtt measured Descartes by his influence on his contemporaries and immediate successors, as well as his influence on the deep metaphysical commitments that moderns came to hold, rather than measure Descartes by what was taken to be the truth of modern physics as Whewell and Mach had done.

> The work of Descartes had an enormous influence throughout all Europe during the latter half of the seventeenth century, largely because he was not only a great mathematician and anatomist, but also a powerful philosophical genius, who treated afresh, and with a remarkably catholic reach, all the big problems of the age by hitching them up in one fashion or another to the chariot of victorious mathematical science.[860]

The Dutch mathematician and historian of science, E. J. Dijksterhuis expanded upon the narrative of mathematization and Descartes's place in it in his work *Mechanization of the World Picture.* According to Dijksterhuis, the increasingly prominent position of mathematics in the seventeenth century for people such as Kepler and Galileo was not merely due to the "indispensable services it rendered" but rather "to the fact that the *structure* of the external world was essentially mathematical in character and a natural harmony existed between the universe and the mathematical thought of the human mind."[861] Dijksterhuis went even further than Burtt with his position on Descartes's place in this trend writing that

> the standpoint taken by Descartes cannot be better described than by saying that by carrying this conception to the extreme he virtually identified mathematics and

[860] Ibid., 125.
[861] Dijksterhuis, *Mechanization* (IV: 194), 404. Italics added.

> natural science. Natural science is mathematical in character not only in the wider sense that mathematics ministers to it, in whatever function this may be, but also in the much stricter sense that the human mind produces the knowledge of nature by its own efforts in the same way as it does mathematics.[862]

To support this position Dijksterhuis made several powerful claims that have since proven to be false, or at least rather unlikely. For example, the claim, also mentioned in outline by Burtt, that analytic mathematics served as the methodology of Descartes's natural science has been criticized, particularly the use of the posthumously published work *Regulae ad Directionem Ingenii* (composed in 1629, but published in 1701) as evidence for this view. Dijksterhuis claimed that the *Regulae* contains "an exposition of the so-called *Mathesis Universalis*, which Descartes always regarded as one of his greatest methodological discoveries and which he wished to see applied in all the natural sciences."[863] It is true that Descartes discussed the idea of a *mathesis universalis* and it bears some similarities to *algebra speciosa* (formulated by Vieta) which resembles the analytic geometry that Descartes himself later developed and published in *La Géométrie* (1637), but Descartes broke off work on the *Regulae* and abandoned the project of a universal mathematics by 1628. He likely did not "always regard [it] as one of his greatest methodological discoveries," had good reasons for abandoning it, and likely did not want to see it applied in all the natural sciences, nor did he in fact apply it.[864] Although evocative, Dijksterhuis's conclusion does not follow: "Thus the aim of the Cartesian method is indeed to cause all scientific thinking to take place in the manner of mathematics, namely by deduction from axioms and by algebraic calculation."[865]

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[862] Ibid,
[863] Ibid., (IV: 195), 405.
[864] John Schuster, "Descartes' *Mathesis Universalis: 1619-28*," in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: The Harvester Press, 1980), 41-96.
[865] Dijksterhuis, *Mechanization* (IV: 195), 405.

Dijksterhuis himself back-peddled a bit in his text, noting that there are "so few marks of it ... to be found in the work of Descartes himself and in particular that his scientific writing contains so few calculations."[866] He claims this is because Descartes is only one piece of a much larger "working-programme" that extended "for many centuries, not for the lifetime of one individual."[867] So, in Dijksterhuis's view Descartes himself foresaw the application of universal mathematics to science, but it "remained hardly more than a remote ideal for Descartes."[868] Nevertheless, Dijksterhuis maintained a narrative that the *real* Cartesian method should be found in an unfinished work that was not published during his lifetime (*Regulae*), and although Descartes published his analytic geometry in an essay (*La Géométrie*) appended to his *Discours de la Méthode* (1637), the methodological rules described in the finished and published *Discours* are not his true method, rather traces of the *mathesis universalis* should be uncovered as the true method from an interpretation of the mathematics found in *La Géométrie.*

In addition to Descartes's important position in the mathematization of nature, Dijksterhuis understandably emphasized Descartes as a "rationalist."[869] According to Dijksterhuis, "physics" for Descartes "is the science of moving forms of space;" and geometry "is concerned with resting forms of space." So, Physics, just like geometry, "can be deduced from axioms established *a priori*. The human mind brings forth not only mathematics but physics as well."[870]

[866] Ibid.
[867] Ibid.
[868] Ibid. (IV: 198), 406.
[869] Dijksterhuis, however, to my knowledge, did not use the now familiar binary "rationalist/empiricist" to refer to and to categorize various early modern philosophies.
[870] Dijksterhuis, *Mechanization* (IV: 199), 406-7.

Like Diederik Korteweg, who edited much of the 16th volume of the *Œuvres complètes de Christiaan Huygens* (1929), Dijksterhuis, who also worked as an editor of Huygens's complete works,[871] emphasized Descartes's work on collision only in so far as it erred and was corrected by Huygens. Like his predecessors Dijksterhuis used modern mathematics to represent collision for both Descartes and Huygens. He was explicit in his choice to use "algebraic symbols" and apparently saw no issue in doing so.[872] The main error Dijksterhuis thought Descartes made is thus explained in concepts that had not yet been developed in Descartes's time: Descartes did not understand velocity as a vector quantity, and thus his concept of the conservation of *quantitas motus* (which Dijksterhuis takes to be identical to a mistaken understanding of momentum) as well as his third law are flawed.

> That the total momentum does not change upon impact is true only if one takes into account the direction of the momentum, and so considers it as a vector. Descartes omits to do so, but only multiplies the mass by the magnitude of the velocity. This makes him think that if the velocity only changes its direction, no change of momentum occurs and that the originally resting body may therefore continue at rest, an assumption which in itself may appear plausible because its tendency to continue at rest is stronger than that of the other to persevere in motion. In reality the momentum of this resting body always has to undergo a change owing to the impact, so that it will undoubtedly start to move; the only case in which this would not happen is if it were kept at absolute rest or—which comes to the same thing—if its mass were infinite.[873]

The relevance of the development of the rules of collision in general in the 17th century for Dijksterhuis is quite similar to Whewell's and Mach's views:

> The subject [specifically Huygens work finding the laws for perfectly elastic impact] deserves our attention for two reasons: in the first place for the ingenious

[871] E. J. Dijksterhuis edited several sections of volume 16 (pages 344-349, 392-412 and 463-469) as well as the first part of volume 20 which treats Musicology as well as pages 344-349.
[872] Dijksterhuis, *Mechanization* (IV: 143), 374. Just before "prov[ing] some of the propositions deduced by Huygens," Dijksterhuis explained that "we will...make use of algebraic symbols for the sake of brevity."
[873] Dijksterhuis, *Mechanization* (IV: 207), 411.

> way in which certain mechanical principles, already discussed above, are used in the derivation, and secondly for the fundamental importance which the phenomenon of impact was to have in the development of the world-picture of seventeenth-century physics.[874]

Like Mach, nothing all that new was achieved in the study of impact, rather it was an instance of the successful use of principles developed previously. Like Whewell, Dijksterhuis claimed that there were obstacles in the way of Galileo's attempts to determine the rules of impact, such as the attempt to measure the moving forces in impact by the continuous static force of weight.[875] Unlike either Mach or Whewell, the significance of the studies of impact was that they came to hold a fundamental place in the development of the *world-picture* of seventeenth-century physics, namely mechanism.

Even if Descartes's work was influential in bringing about a mathematical understanding of the structure of the world, Descartes's attempts to do so were not without criticism. French philosopher and historian Paul Mouy provided an in-depth analysis and criticism of Descartes's physics in *Le développement de la physique cartésienne, 1646-1712*. (Vrin, 1934), calling it "a mathematical physics without mathematics."[876] In 1939 Koyré also criticized Descartes's "mathematization," modifying an older critical trope—the comparison of Descartes with Galileo—but now placing the emphasis on "mathematization":

> [T]he traditional historical view, which sees Galileo as the father of classical science, is not wrong. For it is in his work—and not in that of Descartes—that the

[874] Ibid. (IV: 143), 373. See section c, "The Laws of Perfectly Elastic Impact" (IV: 143-6).
[875] Ibid. (IV: 122-3), 358-9.
[876] Paul Mouy, *Le Développement de la Physique Cartésienne, 1646-1712* (Paris: Vrin, 1934), 144. Alexandre Koyré also notes this: "It is well known that Descartes' physics, as it is set out for us in the *Principes* no longer contains mathematically expressible laws. It is, in fact, no more mathematical than that of Aristotle." Alexandre Koyré, *Galileo Studies*, trans. John Mepham (Atlantic Highlands: Humanities Press, 1978), 90.

> idea of mathematical physics, or rather the idea of the mathematisation of the physical, was realised for the first time in the history of human thought.[877]

Another of Koyré's main criticisms of Descartes, which has had some influence, is that the combination of mechanisms in Descartes's physics was too complicated to express with mathematics. To argue this Koyré points to a letter Descartes sent to Mersenne which discussed the law of fall. In it Descartes wrote that he did not have an answer to "the question concerning the slowing down of a heavy body's motion by the air it is moving in;" Descartes explained that "this depends on so many things that I cannot give a clear account of it in a letter; all I can say is that neither Galileo nor anyone else could work out anything clear and demonstrative on this question unless they first knew what gravity is, and unless they have the true principles of physics." Here Koyré takes Descartes to task: Descartes thought he did have the true principles of physics and he thought he knew what gravity was, but he still could not do it.

> So why then does he not give us the solution? Because it is too complicated. Because in a physics such as his own, a physics of the plenum and of the continuum, everything depends on everything else, everything acts *instantaneously* on everything else. One cannot isolate any phenomenon and as a result one cannot formulate simple laws in mathematical form.[878]

The vortices in the plenum—the very point in Descartes's philosophy that Burtt thought indicated his "wrong turn," and the very aspect of the philosophy that Koyré thought rendered Descartes's physics too complicated to be of any use—provided the context for an extremely influential study of Descartes's rules of collision. This was E. J. Aiton's much quoted 1957 article, "The Vortex Theory of Planetary Motions."[879] This article is significant for several reasons. (1) It includes a sustained and detailed exposition

[877] Koyré, *Galileo Studies*, 201.
[878] Ibid., 93.
[879] E. J. Aiton, "The Vortex Theory of the Planetary Motions—I," *Annals of Science: a quarterly review of the history of science since the renaissance* 13 (1957): 249-264.

of Descartes's third law (the collision law) and his rules of collision in the context of their foundational role for explanations of phenomena by means of material contact in the plenum.[880] (2) It includes a concise diagram of the rules of collision that has been explicitly reproduced by subsequent studies on the topic, which I too have reproduced in chapter 3. (3) Like many before him, Aiton is critical of Descartes's third law (particularly Descartes's notion that the motion of bodies can only be changed by collision with other bodies), but rather than dismiss the law, and rather than focus his study on a comparison of it with the correct formulations of comparable rules derived from contemporary physics, Aiton sought to explain Descartes's position as Descartes himself may have understood it. (4) The article is also significant because Aiton's presentation covers many of the topics that subsequent histories of Descartes's physics have continued to debate:

- The role of experience and experiment in the formulation of Descartes's rules of collision.
- The connection between the rules and the third law, namely the derivation of several rules versus the "interpolation" of other rules. The latter being those rules that involve "equal forces" and cannot, strictly speaking, be derived from the third law, which, as stated, relies on a contest between unequal forces.
- The specific meaning of Descartes's rather ambiguous "force of resistance" in both moving bodies and resting bodies. In Aiton's reconstruction, he models this

[880] Aiton, "Vortex Theory," 249. The context that determines the importance of this sustained and detailed exposition for Aiton is Newtonian dynamics. The Cartesian vortex theory was "formulated before the birth of Newton...[and] was still widely accepted on the continent a decade after Newton's death." To consider the theory as perverse does not do justice to Huygens, Leibniz, Jacques Cassini and John Bernoulli, "each of whom accepted the vortex theory with various modifications in preference to that of Newton. ... It appears evident that the rise of Newtonian celestial dynamics can be seen in historical perspective only when account is taken of the influence of the Cartesian theory." Until Aiton, the vortex theory had received little attention with the exception, as Aiton notes, of Paul Mouy's *Le développement de la physique cartésienne, 1646-1712* (1934) and Piere Brunet.

on an algebraic expression of the Cartesian "quantity of motion." Although this has been influential, I will argue that there is good reason to think this is incorrect.

- The importance of Descartes's notions of "fluidity" and "solidity," and the case of a large body at rest *in a fluid*, which can be moved by the smallest force, and the relationship this has to Descartes's 4th rule which states that a larger body at rest cannot be moved by a smaller body.
- The discrepancy between Descartes's two definitions of motion is discussed as well as the supposed relationship among the definitions, the notion of relative motion, and the controversy regarding the Church's doctrine against the motion of the Earth.

These topics, and Aiton's position on them, are discussed and debated in the course of my study on Descartes's rules of collision.[881]

In 1960 the Department of History and Philosophy of Science was formed at Indiana University. Three years later Richard Westfall joined the faculty. After 7 years he prepared a textbook drawn from his experience teaching the history of science in the 17th century: *The Construction of Modern Science: Mechanisms and Mechanics*, which was first published in 1971.[882] The textbook presents the history of the "scientific revolution" as the conflict and resolution of two trends—the "Platonic-Pythagorean tradition" which maintained that nature was ordered fundamentally according to mathematics and could be best understood in geometrical terms, and the "mechanical philosophy" which maintained that nature was a machine and could be best understood by explaining the mechanisms underlying phenomena. In this textbook presentation, Descartes fulfills an enormously significant role, not in the Platonic-Pythagorean tradition which previous histories (Burtt & Dijksterhuis) emphasizing Descartes's role in mathematization might suggest, but

[881] See chapter 3.
[882] Richard Westfall, *The Construction of Modern Science: Mechanisms and Mechanics* (Cambridge: Cambridge University Press, 1977). The text was first published by John Wiley & Sons, Inc. in 1971.

rather in the mechanical philosophy (as Aiton's emphasis on the vortex theory suggests). Or, as Westfall put it: "what appears to be a spontaneous movement toward a mechanical conception of nature in reaction against Renaissance Naturalism."[883] Although "no one man created the mechanical philosophy" Westfall claimed that

> René Descartes (1596-1650) exerted a greater influence toward a mechanical philosophy of nature than any other man, and for all his excesses, he gave to its statement a degree of philosophic rigor it sorely needed, and obtained nowhere else.[884]

Westfall goes on to explain what he calls Descartes's concept of "inertia" as well as the conservation of quantity of motion, noting (like so many prior historians since at least Whewell) that this "approaches the conservation of momentum" except for Descartes's scalar rather than vector understanding of speed (as has also been pointed out by others such as Dijksterhuis and Koyre).[885] Although Descartes's conclusions "vary widely from those we accept," Descartes's study of collision was important because (1) "Descartes' analysis of impact was the starting point of later efforts that bore more fruit" and (2) "his rules of impact provided the model of all dynamic action; in a mechanical universe shorn of active principles, bodies could act on one another by impact alone."[886]

Similar to Aiton, in the course of his slim text, Westfall addressed a number of important topics that have since become contentious issues. For example, he points out that "Descartes' treatment of impact entangled itself hopelessly in his idea of the *force* of a body's perseverance in its state," and yet Descartes nowhere specified in his mechanics a "clear conception of force."[887] Aiton had provided an interpretation of a specific

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[883] Westfall, *Construction of Modern Science*, 30-1.
[884] Ibid., 31.
[885] Ibid., 32-3, 121-2.
[886] Ibid., 34.
[887] Ibid., 122. Italics added.

force—the "force of resistance," as we have seen. Westfall's point here is about force in general. He flags the issue but does not provide his own interpretation. This issue of "force" and its place (or lack of place) in Descartes's physics has remained a debated issue among interpreters of Descartes.[888] Westfall also addressed the "two-definitions-of-motion-issue" in Descartes and, like Aiton, suggests that Descartes fundamental notion of motion was relative: "rest and motion were relative terms; [*i.e.*] since there is no space apart from bodies, we can only say that a body moves or is at rest in relation to another body."[889] According to Westfall, Descartes rules of collision, then, are *inconsistent* since they "unfortunately yielded different results for different frames of reference."[890] For example, a larger body at rest remains at rest after it is impacted by a smaller body in motion, but a larger moving body will change its motion after impacting a smaller body at rest, specifically, the larger loses as much motion as it gives to the smaller and they both move together after impact.[891] The related positions, (1) that Descartes thought motion and rest were relative and (2) the rules of impact are thus inconsistent, had been argued prior to Westfall. I present them here since with Westfall "relativity of motion" and consequently "inconsistency of the rules of impact" became codified in a textbook presentation. However, there has not been academic agreement on either position. The current entry on Descartes's Physics in the Stanford Encyclopedia of Philosophy by Ed

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[888] For example some historians have claimed that Descartes was ultimately committed to the nonexistence of forces: Dijksterhuis, *Mechanization*; E. J. Aiton, *The Vortex Theory of Planetary Motions* (New York: American Elsevier Inc., 1972); and Daniel Garber, *Descartes' metaphysical physics* (Chicago: University of Chicago Press, 1992). Other's have argued that Descartes must have been committed to the position that forces are inherent in bodies: Martial Gueroult, "The Metaphysics and Physics of Force in Descartes," in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: The Harvester Press, 1980), 196-229; Alan Gabbey, "Force and Inertia in the Seventeenth Century: Descartes and Newton," in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: Harvester Press, 1980), 230-320; and Stephen Gaukroger, *Descartes' System of Natural Philosophy* (New York: Cambridge University Press, 2002).

[889] Westfall, *Construction of Modern Science*, 126.

[890] Ibid.

[891] AT VIII 68-9. *Principia* II 49 and 50.

Slowik valiantly organizes the various possible positions and problems one can take on these topics.[892]

The mechanical philosophy, according to Westfall, became an incredibly influential and pervasive force in the 17th century. "[It] defined the framework in which nearly all creative scientific work was conducted. In its language questions were formulated; in its language answers were given."[893] But, according to Westfall, the mechanical philosophy (and thus Descartes's physics) was an *obstacle* of the Platonic-Pythagorean trend and thus the trend of the mathematization of nature, which would not be resolved until the work of Isaac Newton.[894] Throughout *The Construction of Modern Science* Westfall describes several ways in which the mechanical philosophy was influential and yet an obstacle to mathematization. One way, which is directly related to Descartes's study of impact, is the issue of force for mechanical philosophers such as Descartes. In Westfall's view they were unable "to consider any conception of force except the 'force of a moving body,'" and had no way to understand the forces that *cause* motion. Thus, according to Westfall, they "tended to confine mechanics within kinematic problems" and could not contribute to "the development of a mathematical dynamics."[895] Although "motion," "motion as a cause of phenomena," and "the cause of motion," were all topics of interest to people in the 17th century, the kinematic/dynamic distinction was

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[892] Edward Slowik, "Descartes' Physics," *The Stanford Encyclopedia of Philosophy*, Summer 2014 Edition, ed. Edward N. Zalta. Accessed January 25, 2015. http://plato.stanford.edu/archives/sum2014/entries/descartes-physics/. Also see Edward Slowik, *Cartesian Spacetime: Descartes' Physics and the Relational Theory of Space and Motion* (Boston: Kluwer Academic Publishers, 2002).
[893] Westfall, *Construction of Modern Science*, 41.
[894] Ibid., 42. "Despite its rejection of a qualitative philosophy of nature, the mechanical philosophy in its original form was an obstacle to the full mathematization of nature, and the incompatibility of the two themes of 17th century science was not resolved before the work of Isaac Newton. Meanwhile, virtually no scientific work in the 17th century stood clear of its influence, and most of the work cannot be understood apart from it."
[895] Ibid., 123.

not articulated until much later and can skew our understanding of the manners in which the historical actors understood motion, mechanics, and natural philosophy. In addition, Westfall presumably had in mind the seemingly “occult” notion of “attraction” as a “cause of motion”—a notion which mechanical philosophers would not accept. But it should be noted that Descartes did clearly name the cause of motion in the *Principia*. God is the primary cause of motion, and acting always in the same way, conserves the total quantity of motion. The laws of nature are described by Descartes as the secondary causes of motion.[896]

Alan Gabbey has extensively investigated “the mechanical philosophy” and its relationship to “mechanics.”[897] An influential text from 1980, “Force and inertia in the seventeenth century: Descartes and Newton” takes issue with “textbook simplifications [which] seriously misconstrue, and thereby distort, Descartes’ overall presentation.” In Gabbey’s view it is wrong to think of Descartes “as the arch-mechanist, claiming to describe a world fashioned out of extension and motion alone” with no ontologically robust notion of force.[898] In Part I Gabbey pieces together an ontology of force for Descartes (while acknowledging that there is no systematic presentation of the topic in

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[896] AT IX part 2, 83. *Principes* II 36. "After having examined the nature of movement, we must consider its cause, which is twofold: {we shall begin with} the universal and primary one, which is the general cause of all the movements in the world; and then {we shall consider} the particular ones, by which individual parts of matter acquire movements which they did not previously have. As far as the general {and first} cause is concerned, it seems obvious to me that this is none other than God Himself, who, {being all-powerful} in the beginning created matter with both movement and rest; and now maintains in the sum total of matter, by His normal participation, the same quantity of motion and rest as He placed in it at that time." Translation by Miller, *Principles*, 57. The laws of motion, on the other hand, are secondary and particular causes of the motions of bodies. AT IX part 2, 84-7, *Principes* II 37, 39, and 40. "Furthermore, from this same immutability of God, we can obtain knowledge of the rules or laws of nature, which are the secondary and particular causes of the diverse movements which we notice in individual bodies." Translation by Miller, *Principles*, 59.

[897] Alan Gabbey, "What was 'Mechanical' about the 'Mechanical Philosophy'"? In *The Reception of the Galilean Science of Motion in Seventeenth-Century Europe*, ed. Carla Rita Palmerino and J. M. M. Hans Thijssen (Dordrecht: Kluwer, 2004) 11-23.

[898] Alan Gabbey, “Force and Inertia in the Seventeenth Century: Descartes and Newton,” in *Descartes: Philosophy, Mathematics and Physics*, ed. Stephen Gaukroger (Sussex: Harvester Press, 1980), 234.

Descartes's writings), which depends on a subtle discussion of modes, qualities, and attributes. This is part of a broader comparison with Newton, Leibniz, and Hobbes, but, at least with respect to Descartes, Gabbey concludes that he "does not exclude force from body or its actions."[899] He says in Part II that Descartes's "force to remain at rest" "reveals an empirical awareness of the nature of matter that is closer to the passive forces of the mature Leibniz and, as I hope will emerge in Part II of this study, to the *vis inertiae* of Newton, than the abstract geometrist conceptions normally associated with the founder of Cartesianism."[900] Part II of the text focuses on the "functional meaning of force within a mechanical system" by means of a comparison between the views of Descartes and Newton.[901] It is here that Gabbey explains what he calls the traditional "contest view" of force:

> Taking seventeenth-century 'dynamics' as a whole...the great majority of its practitioners understood force as in its functional sense as that concomitant of a body—expressed in terms of its whole speed and corporeal quantity—which could be identified with the body's relative capacity to overcome a similarly understood resisting force.... Interactions between bodies were seen as contests between opposing forces, the larger forces being the winners, the smaller forces being the losers: a conception of evidently anthropomorphic origin.[902]

He uses this to illustrate differences in view regarding the special case of a body colliding with another at rest. For those people (Hobbes, Malebranche, young Leibniz, etc.) who thought body did not have force, a resting body would have no force of resistance, and thus "the contest notion did not apply, the total available force being redistributed among the bodies according to the conservation principle.

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[899] Gabbey, "Force and Inertia," 238.
[900] Ibid., 238-9.
[901] Ibid., 243.
[902] Ibid. Gabbey cites J. Herival as his source for the 'contest' analogy *á propos* Descartes. John Herival, *The Background to Newton's* Principia*: A Study of Newton's Dynamical Researches in the Years 1664-84* (Oxford: Clarendon Press, 1965), 49. See chapter 3, section 5.2.1 for my interpretation.

> For others (Descartes, Newton and Leibniz in the 1690s), a body at rest reacts against any attempt to set it in motion, thus giving rise to a contest between opposing forces. However, it was Newton who fully realised that the opposing forces must be reinterpreted...so that they are always and necessarily *equal*. ... In other words, Newton saw that the opposing forces actually involved in an interaction do not *determine* the exchanges of motion, as is the case according to the traditional view, but are the dynamical *expression* of these changes.[903]

Gabbey goes on to argue that Descartes's conception of force is midway between the traditional and Newtonian view, although mostly traditional. Thus, he has provided an argument for the point made by Whewell 133 years before—the rules of collision lead to the Newton's third law of motion. In making this argument Gabbey provides an in-depth analysis of Descartes's laws and rules of collision as found in *Le Monde,* the Latin *Principia*, Descartes's 1645 letter to Clerselier, and the French *Principles*.

Gabbey's chapter is found in Stephen Gaukroger's 1980 edited volume on Descartes, which was designed to shift attention away from "purely epistemological discussions of the hyperbolic doubt, the *cogito* and the 'Cartesian circle'" and modern philosophers' attempts to read "modern pre-occupations in epistemology, logic and metaphysics" back into Descartes.[904] Instead, the volume was intended to be "a reassessment of some central issues in Descartes work," particularly his "attempt to provide a philosophical foundation for mathematical physics" and other "issues in Descartes' physics and mathematics."[905] Twenty years later Gaukroger (along with John Schuster and John Sutton) edited another volume on Descartes,[906] informed by the (then) recent but growing scholarship on various less studied aspects of Descartes's work. It too

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[903] Gabbey, "Force and Inertia," 243-244.
[904] Stephen Gaukroger, ed., *Descartes: Philosophy, Mathematics and Physics* (Sussex: The Harvester Press, 1980) ix.
[905] Gaukroger, *Descartes: Philosophy, Mathematics and Physics*, ix.
[906] Stephen Gaukroger, John Schuster, and John Sutton, eds., *Descartes' Natural Philosophy* (New York: Routledge, 2000).

was a “reassessment.” Again, it deliberately neglected the classic “textbook image of Descartes in philosophy or the history of ideas.”[907] But now it deliberately focused—not on Descartes’s physics and mathematics—but on Descartes’s “natural philosophy,” which, depending on how the term is defined, encompassed the mechanical philosophy as well as “mechanics, optics, anatomy, and physiology,” and placed his natural philosophy in its social and intellectual contexts.[908]

Descartes’s rules of collision are still addressed in Gaukroger’s 2000 volume, for example Peter McLaughlin’s chapter, “Force, Determination and Impact,”[909] as well as Daniel Garber’s “A Different Descartes: Descartes and the programme for a mathematical physics in his correspondence.”[910] The latter acknowledges what many prior historians have noted: that Descartes’s physics is in fact qualitative (recall Koyré and Mouy above): his basic conservation law is not given quantitatively, nor is it used in a quantitative way, his notion of centrifugal force which is central to his theory of vortices is never quantified, “his cosmology is entirely qualitative, as was his discussion of the Copernican, Ptolemaic and Tychonic cosmology...

> Descartes’ physics can be read like a novel, as he suggested to the Princess Elisabeth: there are elegant diagrams, and beautiful images, but not one single equation or geometrical argument. The physics of the *Principia* is all words.[911]

But as Garber’s title suggests a “different Descartes,” who is actually engaged in mathematical physics, can be found in his correspondence. Additionally, Gaukroger

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[907] Gaukroger, Schuster, and Sutton, *Descartes' Natural Philosophy*, 1. According to the editors, the "textbook image" among historians of ideas was Descartes “as father of modern philosophy, or as the inventor of modern epistemology, mind/body dualism, or advocate of a universal method.”
[908] Gaukroger, Schuster, and Sutton, *Descartes' Natural Philosophy*, 1.
[909] Peter McLaughlin, "Force, determination and impact," in *Descartes' Natural Philosophy*, ed. Stephen Gaukroger, John Schuster, and John Sutton (New York: Routledge, 2000) 81-112.
[910] Daniel Garber, "A different Descartes: Descartes and the programme for a mathematical physics in his correspondence," in *Descartes’ Natural Philosophy* ed. Stephen Gaukroger, John Schuster, and John Sutton (New York: Routledge, 2000) 113-130.
[911] Garber, "A different Descartes," 114.

(together with Schuster) has an article that defends the notion that Descartes's early work on hydrostatics forms the foundation of his later natural philosophy,[912] and Klaas van Berkel provides a vivid description of Descartes's inspired and later conflict-riddled relationship with Isaac Beeckman.[913] Nevertheless, representative of larger trends in the professional history of early modern science, in Gaukroger's 2000 volume, motion, mechanics, and mathematics is only a small part of a much more complex and nuanced portrait that includes discussions of experimentation, physiology, sensation, imagination and other topics in a more broadly conceived "natural philosophy."

Daniel Garber had been working on Descartes at least since 1979 and published in 1992 an influential "handbook:" *Descartes' metaphysical physics*. The focus of the book is on understanding Descartes's thoughts on matter, motion, and their metaphysical foundations. According to Garber, his text was not intended to be a reinterpretation of Descartes's thought, but rather it is

> a book that pulls together various aspects of Descartes' metaphysical approach to the world of body and presents them in a systematic and coherent way, a kind of handbook of Cartesian physics, a general introduction to the mechanical philosophy as Descartes or a sympathetic but not uncritical contemporary of his might have understood it.[914]

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[912] Stephen Gaukroger and John Schuster, "The hydrostatic paradox and the origins of Cartesian dynamics," *Studies in History and Philosophy of Science* 33 (2002): 535-72.

[913] Klaas van Berkel, "Descartes' Debt to Beeckman: inspiration, cooperation, conflict," in *Descartes' Natural Philosophy* ed. Stephen Gaukroger, John Schuster, and John Sutton (New York: Routledge, 2000) 46-59. Aspects of Descartes's "debt" to Beeckman have been suggested by historians at least since the 1939 publication of the *Journal tenu par Isaac Beeckman de 1604 à 1634. Tome 1: 1604-1619.* Three more volumes of the *Journal* would follow: *Tome 2: 1619-1627* (1942), *Tome 3: 1627-1634* (1945), *Tome 4: Supplément* (1953). Cornelies De Waard edited the *Journal* for publication. Koyré, for example, took up an extended discussion of Beeckman's possible influence on Descartes. See Koyré, *Galileo Studies*, 79-94, 116-8n. The first volume of the *Journal* was published while Koyré's *Etudes Galiléennes* was being printed. Van Berkel takes the perspective of Beeckman rather than Descartes and argues that Descartes's vehement reaction to Beeckman later in life was because he did not want Beeckman publishing his *Journal*, which would have revealed that Descartes's ideas were not original to himself.

[914] Daniel Garber, *Descartes' metaphysical physics* (Chicago: University of Chicago Press, 1992), 3.

As Garber himself notes, the sympathetic but critical "handbook of Cartesian physics" did, however, provide a couple significant reinterpretations. Importantly, Garber reinterprets Descartes's notion of motion. The received view of Descartes's notion was that he advocated "relative motion," which we have encountered in Aiton and Westfall above (but this has been argued by others elsewhere as well), and Descartes's relative notion of motion was due to his attempt to reconcile his Copernicanism with the doctrine of the Church. According to Garber's persuasive arguments, both points are mistaken.[915] Garber's argument and its implications will be discussed more at length below.

In addition to Garber's handbook, *Descartes' Metaphysical Physics* (1992) another important text for my study of Descartes and collision is Gaukroger's *Descartes' System of Natural Philosophy* (2002). Both focus on Descartes's *Principia*, and discuss his other works in relation to this text. However, the two studies differ in emphasis and in their projects. Garber takes the *Principia* to be Descartes's most mature presentation of his thoughts. As such, Garber focuses on sympathetically and critically explaining the organization and consistency of Descartes's philosophy as presented in the *Principia*. Gaukroger, on the other hand, seems to be interested in the ideas and activities behind the *Principia.* This interest in the formulation of ideas may not be surprising since he had published *Descartes: An Intellectual Biography* a few years before in 1995. Additionally Gaukroger notes that the *Principia* project was left incomplete and takes it upon himself to reconstruct the remaining two parts of the project.[916] For instance, regarding the former point, Gaukroger argues for the gradual formulation of the ideas that would come to be the *Principia* by drawing on evidence of Descartes's early projects in mechanics,

[915] Garber, *Descartes' Metaphysical Physics*, 156-96.
[916] Gaukroger, *Descartes' System*, 2-3.

primarily hydrostatics. He then argues that after the 1633 condemnation of Galileo Descartes shifted his attention to the "legitimation" of the natural philosophical ideas that he had already developed. Descartes attempted a variety of approaches, but settled on the systematic presentation that came to be the *Principia*.[917] According to Gaukroger, Descartes was not a metaphysician whose "activity of science" constituted the deduction of his account of the world from *a priori* principles. Rather, through his work on individual problems, like the hydrostatic paradox, Descartes gradually developed a natural philosophy. Later he embarked on a project of "legitimation" for this natural philosophy and did so by providing a metaphysical foundation and re-presenting it systematically in a style similar to a scholastic textbook. Garber's emphasis, on the other hand is on whether or not and to what extent Descartes's metaphysical foundations in fact support his physics.

These different strategies can be illustrated by the manner in which Garber and Gaukroger respectively explain some of the more contentious issues in Descartes's work. For instance, consider the surprisingly complex (and possibly confused) topic of motion and rest in Descartes's *Principia*. Garber stays close to the text and attempts to make sense of Descartes's presentation using the criteria of "explanatory value," *i.e.* motion and rest must be distinct because for motion to have explanatory value in Descartes's system (which as stated in his text, ultimately relies on little more than matter and

[917] Ibid., 32-3. For example, prior to the *Principia,* Descartes experimented with a dialogue form to present his natural philosophy: *La Recherche de la vérité par la lumiere naturelle*. He also seems to have considered writing "a complete textbook of philosophy" which would have appended to the same volume a "traditional textbook with notes at the end of each proposition, in which his own views and those of others are compared. He seems to have decided on Eustachius's *Summa* as the textbook." He considered calling this work, *Summa Philosphiæ*, which in a letter to Constantijn Huygens he noted was the same title as Eustachius's textbook, 'to make it more welcome to the Scholastics, who are now persecuting it and trying to smother it before its birth'." So too did he explain this project to Dinet, a Jesuit who had taught at La Flèche: '... I shall try to use a style more suited to the current practice in the Schools.'"

motion) motion must be distinct from rest.[918] Gaukroger, on the other hand, argues that there are two layers operating behind the text which come to the surface in various sections. The deepest layer relies on two models of motion: one that is “kinematic” and has no distinction between motion and rest, the other which is “dynamic” and is rooted in a model from Descartes’s previous work in (hydro)statics which does have a distinction between motion and rest. The other layer which is closer to the surface (and also has kinematic and dynamic models) derives primarily from the dynamic model in the deepest layer. This, for Gaukroger, makes sense of the surprisingly complex topic of motion and rest in Descartes’s *Principia*.[919]

Similarly, these different strategies can be seen in Garber’s and Gaukroger’s respective approaches to the rules of impact. Gaukroger uses Descartes’s previous work in optics to explain some of the perennially debated topics, such as Descartes’s notorious rule 4 (a small body impacting with a larger body at rest will rebound without moving the larger body).

> “It is not too difficult to understand why Descartes should have insisted on Rule 4, for it underpins his optics, and it is perhaps his realisation that it is needed for his optics that led him to change his mind on this question, for five years earlier he had written to Mersenne on two occasions allowing that a smaller moving body can dislodge a larger stationary one, and even indicating how the resultant speed is determined”[920]

As we will see in the course of my study of collision, this explanation is incorrect for several reasons, but importantly the timeline is off. Garber, on the other hand, seems to downplay the rules of collision altogether, which have been considered a weak point in Descartes’s physics. He opens his chapter on impact by noting that

[918] Garber, *Descartes' Metaphysical Physics*, 156-96.
[919] Gaukroger, *Descartes' System*, 106-14.
[920] Ibid., 126-7.

> Descartes' full treatment of impact was late in coming and, in the end, never fully worked out; the record shows him struggling with the problem, and never really arriving at a single satisfactory view on the problem.[921]

Nevertheless Garber provides a complete and in depth study of the ways Descartes tackled impact in his various texts, noting in particular the changes he makes between the Latin *Principia*, the letter to Clerselier, and the French *Principes*.[922]

[921] Garber, *Descartes' Metaphysical Physics*, 231.
[922] Ibid., 231-262.

Appendix 2
Descartes to de Beaune, 30 April 1639

The text of the 30 April 1639 letter[923] comes from Claude Clerselier's *Lettres de Mr Descartes*, volume 2 (Paris, 1659).[924] Clerselier provided neither a name of destination nor a date for the letter. Adam and Tannery are convinced that it is the "response to Monsieur de Beaune" to which Descartes referred to twice in a letter he sent to Mersenne on the same day.[925] Not only did Clerselier include this letter in his second volume, he "quite wrongly" inserted the very passage in question—regarding a smaller body moving a larger body—in a letter to perhaps the marquis de Newcastle from March/April 1648 in his third volume (Paris, 1667).[926] The editors of *The Correspondance du P. Marin Mersenne* have a lengthy footnote on this passage.[927] Rather than note the apparent contradiction between this letter and Descartes's infamous 4th rule, they discuss "*le théorème de la conservation de la quantité de mouvement*."[928] They note that the theorem of the *conservation de la quantité de mouvement dans le choc*

[923] AT II 541-4. Descartes to [Mr de Beaune], [30 April 1639]. The letter is also collected in the *Correspondence of Mersenne*. MMC 8: 420-2. René Descartes, à (Santpoort), à Florimond Debeaune, à Blois, (30 April 1639).
[924] Claude Clerselier, *Lettres de Mr Descartes*, vol. 2 (Paris: Chez Charles Angot, 1659), 166-8.
[925] AT II 530, 4. Descartes to Mersenne, 30 April 1639. "...and you see something of weight in my response to Mr de Beaulne [sic]." "You see in my response to Monsieur de Beaune why I no longer think that heavy bodies (*corps pesans*) increase (*augmentent*) their speed equally when descending."
[926] Clerselier, *Lettres de Mr* Descartes, vol. 3 (Paris: Chez Charles Angot, 1667) 636-40. Also see MMC 8: 421n. The text of this "1648" letter has been printed in AT V 133-9 as Descartes [to Marquis de Newcastle?], [March or April 1648]. Adam and Tannery note that this letter has neither a name nor a date in Clerselier's *Lettres*, but they have determined that it was likely sent in March or April of 1648, perhaps to the Marquis de Newcastle. The following passages correspond nearly word for word: AT II 543 line 8 through 544 line 2 (of the 30 April 1639 letter to de Beaune); and AT V 135 line 22 through 136 line 13 (of the March/April 1648 letter to Marquis de Newcastle).
[927] MMC 8: 422n.
[928] The editors of MMC claim that this conservation theorem can be found for "soft bodies" in 1618 in Beeckman's *Journal*. Contrary to the editor's claim, Beeckman's mathematical work on "conservation" did not involve "soft bodies," but rather absolutely hard bodies that do not rebound. In addition, Beeckman seems to have been much more interested in the *loss* of motion rather than a mathematical theorem of conservation. He appeals to God's *perpetuo resuscitant et vivificant* to explain why there does not appear to be loss of motion in nature, contrary to the findings of his mathematical investigations. For details, see chapter 2. The editors of MMC point out that Descartes was very familiar with Beeckman's manuscript, which is quite likely.

des corps appeared for the first time in Descartes's *Le Monde*, which was begun in 1630[929], and was used again in a *manière correct* in a letter to Mersenne in December 1639[930] but *il l'appliquera à tort* in another to Mersenne in October 1640.[931]

[929] The work was abandoned in 1633, but published posthumously by Clerselier in 1677.
[930] AT II 626-39. Descartes to Mersenne, 25 Deccember 1639.
[931] AT III 205-221. Descartes to Mersenne, 28 October 1640.

CLXI

Descartes to [M^r de Beaune].

[30 April 1639]

Text from Clerselier, volume II, letter 25, p. 166 -168.

*Without name or date in Clerselier; but it is the "*response to Monsieur de Beaune", *sent at the same time as the preceding letter, where Descartes speaks of two reprises (p. 530, l. 15-16, and p. 534, l. 3-4).*

Sir,

I believe the time that I took to consider your curved lines very well used,[932] not only because I have learned much from it, but also p. 542, l. 1 particularly because you attest to have some satisfaction in it. I thank you for your exact measure of Refractions;[933] ...

... lines 5-16

I would like to be capable of responding to what you p. 542, l.17 want concerning your Mechanics; but even though all of my Physics is nothing other than Mechanics, however, I have never particularly examined

[932] See the above letter CLVI, p. 513, l. 26 to p. 518, l. 6. [AT II 513-18. Descartes to M. de Beaune, 20 February 1639]. [Footnotes reproduced from AT. Comments in square brackets are my own.]

[933] See page 512, l. 14. [AT II 512. Descartes to M. de Beaune, 20 February 1639]

questions that depend on measures of the speed. Your fashion of distinguishing diverse dimensions in movements, and representing them by lines, is without a doubt the best that can be; and as many diverse dimensions can be attributed to each thing, as are found of diverse quantities to measure. Your distinction of three lines of direction, which are parallel, which tend to a center or to several, is very methodical and useful. The Invention of your Curved Lines is very beautiful; and the reason that you p. 543, l. 1 give for the quadruple tension of a cord which makes the octave, is very ingenious and quite true. It only remains to me to tell you what gives me difficulty regarding Speed, and at the same time, what I judge of the nature of Weight, <and> what you name Natural Inertia.[934]

Firstly, I hold that there is a certain Quantity of Motion in all created Matter, which never increases nor decreases; and thus, when a body causes another to move, it loses so much of its movement as it gives of it to it [the other body]: as, when a

[934] If one does not count “&” at the beginning of this phrase, as we have rendered it, it is necessary to consider it as a title inscribed by Descartes in the margin of the following paragraph, and introduced wrongly in the text (Ed.).

stone falls from a high place against the earth, if it does not return and is stopped, I conceive that that just shakes the earth, and thus transfers to it its movement; but if what it moves of the earth contains 1000 times more matter than it, transfering to it all its movement, it only gives to it a 1000th part of it speed. And for that, if two unequal bodies receive so much movement the one as the other, this similar quantity of movement does not give so much speed to the larger as to the smaller, one can say, in a sense, that the more a body contains of matter, the more it has of *Natural Inertia*; to which one can add that a body, which is large can better transfer its movement to other bodies, than a small one can, and that it can be moved less by them. In this way there is one strength of *Inertia*, which depends on the quantity of matter, and another which depends on p. 544, l. 1
the extent of its area.

As for Weight, I imagine nothing other, if not that all subtle Matter which is from here to the Moon, turning very promptly around the Earth, chases towards it [the Earth] all the bodies which cannot move themselves so quickly. For it chases them with such force, that while they have not yet begun to descend, when challenged they descend; for finally if it happens that they descend as quickly as it is moved, it no longer pushes them at all, and if they descend more quickly, it will resist them. From where you can see that there are many things to consider, before one can determine anything concerning Speed, and it is what always diverts me from it; but one can also account for many things by the method of these Principles, which one cannot attain formerly. As for the rest, I would not write to you so freely of these things, as I did not want to discuss this further, because the proof depends on my World,[935] so I only hope that you interpret them favorably, and so I passionately wanted to testify to you what I follow.

[935] [Descartes's treatise, *Le Monde*]

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Abbreviations

AT	Descartes, René. *Oeuvres de Descartes.* 13 vols. Edited by Charles Adam and Paul Tannery, Vrin: Paris, 1897-1913.
Add. MS	Collection of Harriot's manuscripts held at the British Library. Accessed on microfilm at the Max-Planck-Institut für Wissenschaftsgeschichte.
DSB	*Dictionary of Scientific Biography*. Edited by Charles C. Gillipsie. 16 vols. New York: Scribner, 1970-90.
Harley MS	Collection of Harriot's manuscripts held at the British Library. Accessed at the British Library.
HMC	Collection of Harriot's manuscripts held at Petworth House, Sussex. Accessed on microfilm at the Max-Planck-Institut für Wissenschaftsgeschichte.
HOC	Huygens, Christiaan. *Oeuvres Complètes de Christiaan Huygens*. 22 vols. The Hague: Martinus Nijhoff, 1888-1950.
MMC	Mersenne, Marin. *Correspondance du Marin Mersenne*. 17 vols. Paris: G. Beauchesne, 1932-88.
OCH	Oldenburg, Henry. *The Correspondence of Henry Oldenburg*. Edited by A. Rupert Hall and Marie Boas Hall. 1965-86. 13 vols. Vols. 1-9, Madison: University of Wisconsin Press. Vols. 10-11, London: Mansell. Vols. 12-13, London: Taylor and Francis.

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Publications:

"Algebraic Collisions: Challenging Descartes with Cartesian Tools"
Foundations of Science 19 (2014), 35-51 — 2014

Review of "*Thomas Harriot and His World*, edited by Robert Fox"
Aestimatio 10 (2013), 293-313 — 2013

Teaching:

Instructor	HPSC 100	"Science and History of Beer"	IU	Smr '12, Spr '13
	HPSC 200	"Scientific Reasoning"	IU	Spr '10, Spr '12
	HPSC 207	"The Occult in Western Civilization"	IU	2008–'11
	RELS 105D	"Introduction to the Study of Religion"	MSU	Spr '06, Spr '07
AI-ships	PHIL 105	"Critical Thinking and Reasoning"	IU	Fall '13, Fall '07
	PHIL 105	"Elementary Logic"	IU	Fall 2009
	PHIL 250	"Introductory Symbolic Logic"	IU	Spr 2009
	PHIL 100	"Introduction to Philosophy"	IU	Fall 2008
	MATH 118	"Finite Mathematics"	IU	Spr 2008
TA-ships	RELS 105D	"Introduction to the Study of Religion"	MSU	Dr. Malouf
	RELS 110D	"Religion, Conflict, and Politics"	MSU	Dr. Cohen
	RELS 202D	"Hinduism & Buddhism"	MSU	Dr. Sexson
	RELS 203D	"From Taoism to Zen"	MSU	Dr. Sexson
	RELS 217	"Religion and Science"	MSU	Dr. Sexson
	RELS 220IH	"Interpretation of American Religion"	MSU	Dr. Malouf
Tutor	Mathematics	College Algebra through Calculus II	MSU	2003 –'05
	Literacy	Adult Literacy Indy Reads, Indianapolis		2012

Presentations:

"The Mathematics of Collision and the Collision of Mathematics"
Renaissance Studies Roundtable and Reception, Bloomington, IN — Fall 2013

"The Mathematics of Collision in the 17th Century – Thomas Harriot"
Max Planck Institute for the History of Science, Berlin — Spr 2011

"Algebraic Collisions"
HSS Annual Meeting, Montréal, QC — Fall 2010

"Selecting Values Amongst Discrepancies: Mathematics, Measurement, and Error in the 17th Century"
Andrew W. Mellon Foundation Sawyer Seminar, IU — Fall 2010

"Mathematics in the Historiography of the Scientific Revolution"
The Historiography of Mathematics in the Scientific Revolution
Graduate student workshop, IU — Spr 2010

"Darwin and Mental Powers"
Robert Richards Workshop, Indiana University — Fall 2009

"Algebraic Collisions: Challenging Descartes with Cartesian Methods"
Philosophical Aspects of Symbolic Reasoning in Early Modern Science, Ghent — Smr 2009

Research:

British Library
Manuscript Collections — Spr 2011

Max Planck Institute for the History of Science
Department I (Jürgen Renn, Director), Structural Changes in Systems of Knowledge — Spr 2011

The Chymistry of Isaac Newton
Research Assistant to William Newman, Indiana University — Fall 2010

Leiden University Library Special Collections
Christiaan Huygens Collections, Scaliger Institute — Smr 2009

Tel Zahara Archeology Excavation
Volunteer: Nir David, Israel — Smr 2006

Undergraduate Scholars Program
Montana State University — Smr 2005

Pluralism Project
Student Research Affiliate, Harvard University — Smr 2003

Awards & Honors:

Victor E. Thoren Graduate Student Research Fellowship Spr 2015
History and Philosophy of Science, IU

Canadian Social Sciences and Humanities Research Council Grant Fall 2012
"The Language of Nature: Reconsidering the Mathematization of Science" workshop
Sponsored by the Minnesota Center for Philosophy of Science and the Rotman Institute

Max Planck Pre-doctoral Research Fellowship Spr 2011
Max Planck Institute for the History of Science, Berlin

Indiana University Graduate Fellowship 2011–'12
Indiana University, Bloomington

Scaliger Fellow Smr 2009
Scaliger Institute, Leiden University

Best Student:	**Outstanding Undergraduate Student Award** Department of Mathematics, Montana State University	Fall 2005
	Best Philosophy Student Department of History and Philosophy, MSU	2004–'05
	Outstanding Student of Religious Studies Department of History and Philosophy, MSU	2004–'05
Writing:	**"Best Philosophy Paper of the Year" Award** Department of History and Philosophy, MSU	2004
	"Best Religious Studies Paper of the Year" Award Department of History and Philosophy, MSU	2004
Achievement:	**William & Colleen Lee Outstanding Achievement Award** Department of History and Philosophy, MSU	2004–'05
	MSU Women's Center's Student of Achievement Award MSU Women's Center and the Office of Alumni Affairs	Spr 2005
	Pi Mu Epsilon National Honorary Mathematics Society Montana Beta Chapter	Fall 2005
Leadership:	**MSU Rotary Student of the Year** Rotary Club	2004–'05
	MSU Rotary Student of the Month Bozeman Rotary Club	Feb. 2005
	William & Colleen Lee Award for Leadership Department of History and Philosophy, MSU	2004
Other Honors:	**Nomination for the Dean's Award for Excellence** Nominated by the Department of History and Philosophy, MSU	Spr 2005
	Landis Scholarship Department of History and Philosophy, MSU	2003
	Dean's List, 9 Semesters Mathematics and Philosophy, MSU	2000–'05

Guest Lectures:

Pliny the Elder Spr 2010
HPSC 326 *Divide and Conquer: Natural Order & Classification in Western Thought*
Mundane Miracles in *Deep River* Fall 2006
RELS 402 *Natural, Unnatural, Supernatural*
Ezra, Nehemiah, & the Return from Exile Fall 2006
RELS 204 *Introduction to the Hebrew Bible*
Plotinus & the Soul Spr 2006
RELS 410 *Psyche and The Sacred*
Ten Ox-herding Pictures Spr 2005
RELS 203 *Asian Religions: From Taoism To Zen*
The Eightfold Path Fall 2004
RELS 202 *Asian Religions: Hinduism & Buddhism*

Activities:

High Flyers Trapeze, Catcher 2013–'15
Indy Reads, Volunteer Literacy Tutor 2012
Marathons, Half Marathons, Trail Runs 2005–'15
Kentucky Derby Marathon, Geist Half, Indy Mini, Eagle Creek, Bridger Ridge Run
Students Against Sexual Assault, Co-president 2004–'05
Outreach branch of VOICE (Victims Options in the Campus Environment)
Men Stopping Rape, President 2004–'05
Outreach and education directed toward men on issues of gender and sexual violence
MSU Philosophy Club, President 2004–'05

Other Work:

Bloomington Dog Runner, LLC, Bloomington, IN 2013–'15
Business Owner and Dog Runner
Bloomington Brewing Company, Bloomington, IN 2011–'12
Assistant Brewer
The Kinsey Institute, Bloomington, IN 2007–'09
Library User Services
Vargo's Jazz City and Bookstore, Bozeman, MT 2005–'07
Local independently owned new and used books and music store
Construction, Livingston, MT Smr 2006
Framing, Drywall, Texture, Painting, Carpentry
Montana Harvest, Bozeman, MT 2004–'05
Local Natural Food and Health Grocery Store, produce and bulk departments
VOICE Center, Montana State University 2004–'05
Victims Options in the Campus Environment (VOICE), Student Coordinator
Sacks of Bozeman Thrift Store, Bozeman, MT 2002–'03
Non-profit supporting the Help Center and the Sexual Assault Counseling Center
On The Rise, Bozeman, MT Smr 2002
Sweets Baker – Cinnamon rolls, brownies, cookies, scones, coffee cakes, various sweet breads
Front Desk Clerk, Montana State University 2001–'02
Desk clerk for North Hedges Residence Hall
S. J. Perry Company, Butte, MT 1999–'00
Delivered wholesale industrial plumbing and heating supplies across Montana especially to the various Hutterite Colonies